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Washn.
Apr 27, 1896

BULLETIN

OF THE

PHILOSOPHICAL SOCIETY

OF

WASHINGTON

VOL. 12

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THE PHILOSOPHICAL SOCIETY

OF

WASHINGTON

CONSTITUTION, RULES

LIST OF

OFFICERS AND MEMBERS FOR 1895

AND LIST OF

PRESIDENTS OF THE SOCIETY

CONSTITUTION

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

Article I. The name of this Society shall be The Philosophical Society of Washington.

Article II. The officers of the Society shall be a President, four Vice-Presidents, a Treasurer, and two Secretaries.

Article III. There shall be a General Committee, consisting of the ex-Presidents of the Society, the officers of the Society, and nine other members.

Article IV. The officers of the Society and the nine other members of the General Committee shall be elected annually by ballot; they shall hold office until their successors are elected, and shall have power to fill vacancies.

Article V. It shall be the duty of the General Committee to make rules for the government of the Society, and to transact all its business.

Article VI. This Constitution shall not be amended except by a three-fourths vote of those present at an annual meeting for the election of officers, and after notice of the proposed change shall have been given in writing at a stated meeting of the Society at least four weeks previously.

STANDING RULES

FOR THE GOVERNMENT OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

1. The Stated Meetings of the Society shall be held at 8 o'clock P. M. on every alternate Saturday; the place of meeting to be designated by the General Committee.

2. Notice of the time and place of meeting shall be sent to each member by one of the Secretaries.

When necessary, Special Meetings may be called by the President.

3. The Annual Meeting for the election of officers shall be the last stated meeting in the month of December.

The order of proceedings (which shall be announced by the Chair) shall be as follows:

First, the reading of the minutes of the last Annual Meeting.

Second, the presentation of the annual reports of the Secretaries, including the announcement of the names of members elected since the last annual meeting.

Third, the presentation of the annual report of the Treasurer.

Fourth, the announcement of the names of members who, having complied with section 14 of the Standing Rules, are entitled to vote on the election of officers.

Fifth, the election of President.

Sixth, the election of four Vice-Presidents.

Seventh, the election of Treasurer.

Eighth, the election of two Secretaries.

Ninth, the election of nine members of the General Committee.

Tenth, the consideration of Amendments to the Constitution of the Society, if any such shall have been proposed in accordance with article VI of the Constitution.

Eleventh, the reading of the rough minutes of the meeting.

4. Elections of officers are to be held as follows:

In each case nominations shall be made by means of an informal ballot, the result of which shall be announced by the Secretary; after which the first formal ballot shall be taken.

In the ballot for Vice-Presidents, Secretaries, and Members of the General Committee, each voter shall write on one ballot as many names as there are officers to be elected, viz., four on the first ballot for Vice-Presidents, two on the first for Secretaries, and nine on the first for Members of the General Committee, and on each subsequent ballot as many names as there are persons yet to be elected; and those persons who receive a majority of the votes cast shall be declared elected: *Provided*, That the number of persons receiving a majority does not exceed the number of persons to be elected, in which case the vacancies shall be filled by the candidates receiving the highest majorities.

If in any case the informal ballot result in giving a majority for any one, it may be declared formal by a majority vote.

5. The Stated Meetings, with the exception of the annual meeting, shall be devoted to the consideration and discussion of scientific subjects.

The Stated Meeting next preceding the Annual Meeting shall be set apart for the delivery of the President's Annual Address.

6. Sections representing special branches of science may be formed by the General Committee upon the written recommendation of twenty members of the Society.

7. Persons interested in science, who are not residents of the District of Columbia, may be present at any meeting of the Society, except the annual meeting, upon invitation of a member.

8. On request of a member, the President or either of the Secretaries may, at his discretion, issue to any person a card of invitation to attend a specified meeting. Five cards of invitation

to attend a meeting may be issued in blank to the reader of a paper at that meeting.

9. Invitations to attend during three months the meetings of the Society and participate in the discussion of papers, may, by a vote of nine members of the General Committee, be issued to persons nominated by two members.

10. Communications intended for publication under the auspices of the Society shall be submitted in writing to the General Committee for approval.

11. Any paper read before a Section may be repeated, either entire or by abstract, before a general meeting of the Society, if such repetition is recommended by the General Committee of the Society.

12. It is not permitted to report the proceedings of the Society or its Sections for publication, except by authority of the General Committee.

13. New members may be proposed in writing by three members of the Society for election by the General Committee; but no person shall be admitted to the privileges of membership unless he signifies his acceptance thereof in writing, and pays his dues to the Treasurer, within two months after notification of his election.

14. Each member shall pay annually to the Treasurer the sum of five dollars, and no member whose dues are unpaid shall vote at the annual meeting for the election of officers, or be entitled to a copy of the Bulletin.

In the absence of the Treasurer, the Secretary is authorized to receive the dues of members.

The names of those two years in arrears shall be dropped from the list of members.

Notice of resignation of membership shall be given in writing to the General Committee through the President or one of the Secretaries.

15. The fiscal year shall terminate with the Annual Meeting.

16. Any member who is absent from the District of Columbia for more than twelve consecutive months may be excused from payment of dues during the period of his absence, in which case he will not be entitled to receive announcements of meetings or current numbers of the Bulletin.

17. Any member not in arrears may, by the payment of one hundred dollars at any one time, become a life member, and be relieved from all further annual dues and other assessments.

All moneys received in payment of life membership shall be invested as portions of a permanent fund, which shall be directed solely to the furtherance of such special scientific work as may be ordered by the General Committee.

STANDING RULES

OF

THE GENERAL COMMITTEE

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

With Amendments Adopted April 14, 1888.

---

1. The President, Vice-Presidents, and Secretaries of the Society shall hold like offices in the General Committee.

2. The President shall have power to call special meetings of the Committee, and to appoint Sub-Committees.

3. The Sub-Committees shall prepare business for the General Committee, and perform such other duties as may be entrusted to them.

4. There shall be two Standing Sub-Committees: one on Communications for the Stated Meetings of the Society, and another on Publications.

5. The General Committee shall meet at half-past seven o'clock on the evening of each Stated Meeting, and by adjournment at other times.

6. Six members shall constitute a quorum for all purposes, except for the amendment of the Standing Rules of the Committee or of the Society, in which case a majority of the General Committee shall constitute a quorum.

7. The names of proposed new members recommended in conformity with Section 13 of the Standing Rules of the Society may be presented at any meeting of the General Committee, but shall

lie over for at least four weeks before final action. At least fifteen ballots shall be cast to determine an election. Blanks shall not be counted as ballots. Affirmative ballots to the number of four-fifths of those cast shall be necessary to an election. No rejected candidate shall be eligible to membership within twelve months from the date of rejection. The Secretary of the General Committee shall keep a chronological register of the elections and acceptances of members.

8. These Standing Rules, and those for the government of the Society, shall be modified only with the consent of a majority of the members of the General Committee, but by unanimous consent of a quorum any rule except numbers 6 and 7 of the Standing Rules of the General Committee may be temporarily suspended.

STANDING RULES

OF THE

MATHEMATICAL SECTION.

---

1. The object of this Section is the consideration and discussion of papers relating to pure or applied mathematics.

2. The special officers of the Section shall be a Chairman and a Secretary, who shall be elected at the first meeting of the Section in each year, and discharge the duties usually attaching to those offices.

3. To bring a paper regularly before the Section it must be submitted to the Standing Committee on Communications for the stated meetings of the Society, with the statement that it is for the Mathematical Section.

4. Meetings shall be called by the Standing Committee on Communications whenever the extent or importance of the papers submitted and approved appear to justify it.

5. All members of the Philosophical Society who wish to do so may take part in the meetings of this Section.

6. To every member who shall have notified the Secretary of the General Committee of his desire to receive them, announcements of the meetings of the Section shall be sent by mail.

7. The Section shall have power to adopt such rules of procedure as it may find expedient.

RULES RESPECTING PUBLICATIONS

OF

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

Adopted December 22, 1888.

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1. The regular publication of the Society shall have the form of a series of completed papers or memoirs, to which the transactions of the Society shall be added. Publication shall not be made at stated intervals, but whenever directed by the General Committee.

2. Each paper read before the Society and offered for publication shall be at once referred to a special committee of two appointed by the President, which shall submit to the General Committee at its next meeting a written report on the paper, and the General Committee shall decide respecting its publication. The annual address of the retiring President and the annual reports of the Treasurer and Secretaries shall be published in full, without reference. The journal of the Society shall be published in condensed form at the end of the volume.

3. Papers read before a Section of the Society and offered for publication shall be referred to a committee appointed as the Section may direct. The paper, accompanied by a written report, shall be laid before the General Committee, which shall decide respecting publication.

4. Papers approved by the General Committee for publication shall be printed forthwith, and one hundred copies shall be gratuitously furnished to the author.

5. The papers published from time to time shall be paged consecutively, and when sufficient material has accumulated to form a volume of convenient size, a title page, table of contents, and index shall be prepared, and the whole issued as a volume of the Bulletin of the Philosophical Society.

PRESIDENTS OF THE SOCIETY.

*JOSEPH HENRY	1871–'78.
SIMON NEWCOMB	1879–'80.
*J. J. WOODWARD	1881.
*W. B. TAYLOR	1882.
J. W. POWELL	1883.
*J. C. WELLING	1884.
ASAPH HALL	1885.
J. S. BILLINGS	1886.
WM. HARKNESS	1887.
*GARRICK MALLERY	1888.
J. R. EASTMAN	1889.
C. E. DUTTON	1890.
T. C. MENDENHALL	1891.
G. K. GILBERT	1892.
G. BROWN GOODE	1893.
ROBERT FLETCHER	1894.
W. H. DALL	1895.

* Deceased.

OFFICERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON, 1895.

(Elected December 22, 1894.)

President	W. H. Dall.	
Vice-Presidents	Marcus Baker.	F. W. Clarke.
	F. H. Bigelow.	Lester F. Ward.
Treasurer	W. A. De Caindry.	
Secretaries	B. R. Green.	W. C. Winlock.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

Cyrus Adler. M. W. Harrington.
Henry H. Bates. H. M. Paul.
T. H. Bean. C. V. Riley.
J. H. Gore. O. H. Tittmann.
C. D. Walcott.

STANDING COMMITTEES.

On Communications:

O. H. Tittmann, *Chairman.* F. H. Bigelow. J. R. Eastman.
Henry Gannett. C. H. Merriam.

On Publications:

Marcus Baker, *Chairman.* W. A. De Caindry. W. C. Winlock.

Members of the Joint Commission:

W. H. Dall. Marcus Baker. B. R. Green.

LIST OF MEMBERS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON,

TOGETHER WITH

YEAR OF ADMISSION TO THE SOCIETY, POST-OFFICE ADDRESS, AND RESIDENCE.

Corrected to August, 1895.

1871. Abbe, Cleveland,
Weather Bureau. 2017 I street.

1875. Abert, S. T. (Silvanus Thayer),
722 Seventeenth street. 1738 I street.

1881. Adams, Henry,
1603 H street.

1893. Adler, Cyrus,
Smithsonian Institution. 943 K street.

1876. Baker, Marcus,
Geological Survey. 1905 Sixteenth street.

1871. Bates, Henry H. (Henry Hobart),
The Portland.

1886. Bates, Dr. N. L. (Newton Lemuel), U. S. N.,
Navy Department. The Shoreham.

1888. Bauer, Louis A. (Louis Agricola),
University of Chicago, Chicago, Illinois.

1884. Bean, T. H. (Tarleton Hoffman),
New York Aquarium, Staten Island, New York.

1875. Beardslee, Capt. L. A. (Lester Anthony), U. S. N.,
Navy Department.

1879. Bell, A. Graham (Alexander Graham),
Volta Bureau, 1601 35th street. 1331 Connecticut ave.

1881. Bell, C. A. (Chichester Alexander),
3 Mansfield place, Richmond Hill, England.

1886. Beyer, Dr. H. G. (Henry Gustav), U. S. N.,
U. S. Naval Academy, Annapolis, Md.

1890. Bigelow, Frank H. (Frank Hagar),
Weather Bureau. 1625 Massachusetts avenue.

1871. Billings, Dr. J. S. (John Shaw), U. S. A.,
Army Medical Museum. 3027 N street.

1876. Birnie, Capt. Rogers, U. S. A.,
Ordnance Office, War Dept. 1341 New Hampshire ave.

1892. Blount, H. F. (Henry Fitch),
1405 G street. "The Oaks," 3101 U street.

1884. Bowles, Nav. Con'r. F. T. (Francis Tiffany), U. S. N.,
Navy Yard, Brooklyn, N. Y.

1884. Brown, Prof. S. J. (Stimson Joseph), U. S. N.,
Naval Observatory.

1886. Bryan, J. H. (Joseph Hammond),
818 Seventeenth street. 1644 Connecticut ave.

1879. Burnett, Swan M. (Swan Moses),
906 Farragut square.

1874. Busey, Samuel C. (Samuel Clagett),
1545 I street. 901 Sixteenth street.

1891. Carr, W. K. (William Kearney),
1008 F street. 1413 K street.

1871. Casey, Gen. Thomas Lincoln, U. S. A.,
War Department. 1419 K street.

1882. Caziarc, Capt. Louis V. (Louis Vasmar), U. S. A.,
Artillery School, Fort Monroe, Va.

1883. Chamberlin, T. C. (Thomas Chrowder),
University of Chicago, Chicago, Illinois.

1885. Chatard, Thos. M. (Thomas Marean),
1714 Rhode Island avenue.

1874. Chickering, J. W. (John White),
Deaf Mute College. 2 Kendall Green.

1880. Christie, Alex. S. (Alexander Smyth),
Columbian University.

1877. Clark, Edward,
Architect's Office, Capitol. 417 Fourth street.

1874. Clarke, F. W. (Frank Wigglesworth),
Geological Survey. 1612 Riggs place.

1880. Comstock, J. H. (John Henry),
Cornell University, Ithaca, N. Y.

1879. Craig, Thomas,
Johns Hopkins University, Baltimore, Md.

1889. Cross, Whitman,
Geological Survey. 730 Seventeenth street.

1871. Dall, Wm. H. (William Healey),
Smithsonian Institution. 1119 Twelfth street.

1880. Davis, Comdr. C. H. (Charles Henry), U. S. N.,
Navy Department.

1872. Dean, Dr. R. C. (Richard Crain), U. S. N.,
Navy Department. 1736 I street.

1881. De Caindry, Wm. A. (William Augustin),
Commissary General's Office, War Dept. 1816 H street.

1884. Dewey, Fred. P. (Frederic Perkins),
621 F street. Lanier Heights.

1884. Diller, J. S. (Joseph Silas),
Geological Survey. 1454 Staughton street.

1876. Doolittle, M. H. (Myrick Hascall),
Coast and Geodetic Survey. 1925 I street.

1872. Dutton, Maj. C. E. (Clarence Edward), U. S. A.,
San Antonio, Texas.

1890. Eakins, L. G. (Lincoln Grant),
Care of Phila. S. and R. Co., Pueblo, Colorado.

1884. Earll, R. Edward (Robert Edward),
National Museum. Chevy Chase, Md.

1871. Eastman, Prof. J. R. (John Robie), U. S. N.,
Naval Observatory. 1905 N street.

1888. Edes, R. T. (Robert Thaxter),
2 Commonwealth avenue, Boston, Mass.

1884. Eimbeck, William,
Coast and Geodetic Survey. 1014 Fourteenth street.

1871. Eldridge, Stuart,
Yokohama, Japan.

1873. Endlich, F. M. (Frederic Miller),
Ouray, Colorado.

1874. Ewing, Hugh,
"Idleside," Lancaster, Ohio.

1876. Farquhar, Edward,
Patent Office Library. Cosmos Club, 1520 H street.

1881. Farquhar, Henry,
Department of Agriculture. Bunker Hill road.

1889. Fassig, O. L. (Oliver Lanard),
Weather Bureau. 1424 Eleventh street.

1887. Fernow, B. E. (Bernhard Eduard),
Department of Agriculture. 1303 P street.

1890. Fischer, E. G. (Ernst George),
Coast and Geodetic Survey. 436 New York avenue.

1893. Fischer, L. A. (Louis Albert),
Coast and Geodetic Survey. 33 New York avenue.

1873. Fletcher, Robert,
Army Medical Museum. The Portland.

1882. Flint, A. S. (Albert Stowell),
Washburn Observatory, Madison, Wisconsin.

1881. Flint, Dr. J. M. (James Milton), U. S. N.,
Smithsonian Institution. The Portland.

1875. Gallaudet, E. M. (Edward Miner),
Deaf Mute College, Kendall Green.

1874. Gannett, Henry,
Geological Survey. 1881 Harewood avenue.

1893. Garriott, E. B. (Edward Bennett),
Weather Bureau. 931 New Hampshire avenue.

1894. Gates, Elmer,
1728 New Jersey avenue.

1891. Gihon, Dr. Albert L. (Albert Leary), U. S. N.,
United States Naval Hospital, D. C.

1873. Gilbert, G. K. (Grove Karl),
Geological Survey. 1424 Corcoran street.

1879. Godding, W. W. (William Whitney),
Government Hospital for the Insane.

1885. Gooch, F. A. (Frank Austin),
Yale College, New Haven, Connecticut.

1874. Goode, G. Brown (George Brown),
Smithsonian Institution. Lanier Heights.

1875. Goodfellow, Edward,
1718 Corcoran street.

1886. Gordon, J. C. (Joseph Claybaugh),
Deaf Mute College, Kendall Green.

1880. Gore, J. Howard (James Howard),
Columbian University. 1521 New Hampshire ave.

1878. GRAVES, W. H. (Walter Hayden),
Bureau of Indian Affairs.

1880. GREELY, GEN. A. W. (Adolphus Washington), U. S. A.,
War Department. 1914 G street.

1879. GREEN, BERNARD R. (Bernard Richardson),
Building for Library of Congress. 1738 N street.

1875. GREEN, COMDR. F. M. (Francis Mathews), U. S. N.,
P. O. Building, Boston, Mass.

1871. GREENE, PROF. B. F. (Benjamin Franklin), U. S. N.,
West Lebanon, New Hampshire.

1875. GREENE, FRANCIS VINTON,
No. 1 Broadway, New York, N. Y.

1889. HAGEN, J. G. (John George),
Georgetown College Observatory.

1879. HAINS, LT. COL. P. C. (Peter Conover), U. S. A.,
War Department.

1871. HALL, PROF. ASAPH, U. S. N.,
Naval Observatory. 2715 N street.

1884. HALL, ASAPH, JR.,
Detroit Observatory, Ann Arbor, Mich.

1885. HALLOCK, WILLIAM,
Columbia College, New York, N. Y.

1871. HARKNESS, PROF. WILLIAM, U. S. N.,
Naval Observatory. Cosmos Club, 1520 H street.

1891. HARRINGTON, MARK W. (Mark Walrod),
Seattle, Washington.

1891. HARRIS, R. A. (Rollin Arthur),
Coast and Geodetic Survey. 49th and Albany sts.

1880. HASSLER, F. A. (Ferdinand Augustus),
Santa Ana, Orange County, California.

1886. HAYDEN, EVERETT,
1515 H street. 1802 Sixteenth street.

1889. HAYFORD, J. F. (John Fillmore),
Coast and Geodetic Survey. 1208 M street.

1882. HAZEN, H. A. (Henry Allen),
Weather Bureau. 1422 Eleventh street.

1894. HEDRICK, J. T. (John Thompson),
Georgetown College Observatory.

1874. HENSHAW, H. W. (Henry Wetherbee),
In care of Bureau of Ethnology.

1879. Hill, G. W. (George William),
West Nyack, New York.

1886. Hill, R. T. (Robert Thomas),
Geological Survey.

1884. Hitchcock, Romyn,
1201 New York Life Building, Chicago, Illinois.

1885. Hodgkins, H. L. (Howard Lincoln),
Columbian University. 1830 T street.

1873. Holden, E. S. (Edward Singleton),
Lick Observatory, Mt. Hamilton, California.

1890. Hollerith, Herman,
1054 Thirty-first street. 1521 Thirty-first street.

1887. Holmes, Jesse H. (Jesse Herman),
George School, Newtown, Penn.

1879. Holmes, W. H. (William Henry),
Columbian Museum, Chicago, Illinois.

1874. Howell, Edwin E. (Edwin Eugene),
612 Seventeenth street.

1885. Iddings, Joseph P. (Joseph Paxson),
University of Chicago, Chicago, Illinois.

1891. James, J. N. (John Nelson),
Naval Observatory. 1828 H street.

1880. James, Owen,
Hatboro, Pennsylvania.

1879. Johnson, Joseph Taber,
1728 K street. 924 Seventeenth street.

1884. Kauffmann, S. H. (Samuel Hay),
1101 Pennsylvania avenue. 1421 Massachusetts ave.

1884. Kerr, Mark B. (Mark Brickell),
402 Front street, San Francisco, California.

1880. Kilbourne, Maj. C. E. (Charles Evans), U. S. A.,
Walla Walla, Washington.

1875. King, A. F. A. (Albert Freeman Africanus),
1315 Massachusetts avenue.

1887. Knowlton, F. H. (Frank Hall),
National Museum. 204 A street SE.

1882. Kummell, C. H. (Charles Hugo),
Coast and Geodetic Survey. 608 Q street.

1887. Langley, S. P. (Samuel Pierpont),
Smithsonian Institution. Metropolitan Club.

1884. LAWRENCE, WILLIAM,
Bellefontaine, Ohio.

1871. LINCOLN, N. S. (Nathan Smith),
1514 H street.

1895. LINDENKOHL, ADOLPH,
Coast and Geodetic Survey. 19 Fourth street SE.

1889. LITTLEHALES, G. W. (George Washington),
Hydrographic Office. 928 Twenty-third street.

1886. MCADIE, ALEXANDER,
Weather Bureau.

1891. MCCAMMON, JOS. K. (Joseph Kay),
1420 F street. 1324 Nineteenth street.

1893. MCCORMICK, J. H. (John Henry),
1225 L street.

1883. MCGEE, W J,
Bureau of Ethnology. 1225 Connecticut avenue.

1879. MCGUIRE, F. B. (Frederick Bauders),
1419 G street. 1333 Connecticut avenue.

1876. MCMURTRIE, WILLIAM,
106 Wall street, New York, N. Y.

1884. MAHER, JAMES A. (James Arran),
Roan Mountain, Tennessee.

1885. MANN, B: PICKMAN (Benjamin Pickman),
Patent Office. 1918 Sunderland place.

1886. MARTIN, ARTEMAS,
Coast and Geodetic Survey. 1534 Columbia street.

1885. MARVIN, C. F. (Charles Frederick),
Weather Bureau. 1923 Thirteenth street.

1878. MARVIN, J. B. (Joseph Badger),
Rome, Georgia.

1884. MATTHEWS, DR. WASHINGTON, U. S. A.,
Fort Wingate, New Mexico.

1885. MENDENHALL, T. C. (Thomas Corwin),
Polytechnic Institute, Worcester, Mass.

1886. MERRIAM, C. HART (Clinton Hart),
Department of Agriculture. 1919 Sixteenth street.

1889. MINDELEFF, COSMOS,
1406 Staughton street.

1886. MITCHELL, HENRY,
54 Burroughs street, Jamaica Plain, Mass.

1891. Morton, Geo. L. (George Luton),
Patent Office. 1310 Q street.

1885. Moser, Lt. Com'r J. F. (Jefferson Franklin), U. S. N.,
Coast and Geodetic Survey. 501 Stanton place.

1884. Murdoch, John,
Pocksha Farm, Rock, Mass.

1871. Newcomb, Prof. Simon, U. S. N.,
Navy Department. 1620 P street.

1879. Nordhoff, Charles,
Coronado, San Diego county, California.

1884. Norris, Dr. Basil, U. S. A.,
Occidental Hotel, San Francisco, California.

1885. Nott, Charles C. (Charles Cooper),
Court of Claims. 826 Connecticut avenue.

1884. Ogden, H. G. (Herbert Gouverneur),
Coast and Geodetic Survey. 1809 Eighteenth street.

1878. Osborne, J. W. (John Walter),
216 Delaware avenue NE.

1871. Parke, Gen. John G. (John Grubb), U. S. A.,
16 Lafayette square.

1877. Paul, H. M. (Henry Martyn),
Naval Observatory. 2201 K street.

1895. Pawling, Jesse, Jr.,
Coast and Geodetic Survey. 1616 Fifteenth street.

1874. Peale, A. C. (Albert Charles),
1451 Staughton street.

1873. Poe, Gen. O. M. (Orlando Metcalfe), U. S. A.,
131 East Congress street, Detroit, Michigan.

1884. Poindexter, W. M. (William Mundy),
806 Seventeenth street.

1882. Pope, Dr. B. F. (Benjamin Franklin), U. S. A.,
Angel Island, California.

1874. Powell, J. W. (John Wesley),
Bureau of Ethnology. 910 M street

1892. Powell, W. B. (William Bramwell),
Franklin School. 1410 N street.

1880. Prentiss, D. W. (Daniel Webster),
1101 Fourteenth street.

1888. Preston, E. D. (Erasmus Darwin),
Coast and Geodetic Survey. 902 Mass. ave. NE.

1879. PRITCHETT, H. S. (Henry Smith),
Observatory, Washington University, St. Louis, Mo.

1892. PUTNAM, G. R. (George Rockwell),
Coast and Geodetic Survey. 1314 Thirteenth street.

1882. RATHBUN, RICHARD,
U. S. Fish Commission. 1622 Massachusetts avenue.

1884. RAY, CAPT. P. H. (Patrick Henry), U. S. A.,
Shoshone Agency, Wyoming.

1884. RICKSECKER, EUGENE,
P. O. Box 289, Seattle, Washington.

1878. RILEY, C. V. (Charles Valentine),
National Museum. Sunbury, Wyoming avenue.

1879. RITTER, W. F. McK. (William Francis McKnight),
P. O. Box 450, Milton, Pennsylvania.

1884. ROBINSON, THOMAS,
Fort Mitchell, Alabama. Vienna, Virginia.

1872. ROGERS, JOSEPH A. (Joseph Addison),
Naval Observatory.

1882. RUSSELL, ISRAEL C. (Israel Cook),
University of Michigan, Ann Arbor, Mich.

1883. SAMPSON, CAPT. W. T. (William Thomas), U. S. N.,
Navy Department. 1613 New Hampshire avenue.

1871. SAVILLE, J. H. (James Hamilton),
1419 F street. 1420 Seventeenth street.

1871. SCHOTT, C. A. (Charles Anthony),
Coast and Geodetic Survey. 212 First street SE.

1890. SEARLE, G. M. (George Mary),
Catholic University of America, Brookland, D. C.

1875. SHELLABARGER, SAMUEL,
Kellogg Building. 812 Seventeenth street.

1874. SHERMAN, JOHN,
U. S. Senate. 1321 K street.

1895. SHIDY, L. P. (Leland Perry),
Coast and Geodetic Survey. 1617 Marion street.

1881. SHUFELDT, R. W. (Robert Wilson),
Smithsonian Institution. Takoma, D. C.

1883. SKINNER, DR. J. O. (John Oscar), U. S. A.,
Chambersburg, Pa.

1882. SMILEY, CHAS. W. (Charles Wesley),
Inventive Age Building. 943 Massachusetts avenue.

1891. SMILLIE, THOS. W. (Thomas Wilson),
National Museum. 1808 R street.

1876. SMITH, CHF. ENG. DAVID, U. S. N.,
Navy Department. 1714 Connecticut avenue.

1880. SMITH, EDWIN,
Rockville, Maryland.

1887. SMYTH, H. L. (Henry Lloyd),
16 Oxford street, Cambridge, Mass.

1872. SPOFFORD, A. R. (Ainsworth Rand),
Library of Congress. 1621 Massachusetts avenue.

1890. STANLEY-BROWN, JOSEPH,
1318 Massachusetts avenue.

1893. STANTON, T. W. (Timothy William),
National Museum. 205 Thirteenth street SW.

1874. STONE, ORMOND,
University Station, Charlottesville, Va.

1881. TAYLOR, F. W. (Frederick William),

1875. THOMPSON, A. H. (Almon Harris),
Geological Survey. 1729 Twelfth street.

1884. THOMPSON, GILBERT,
Geological Survey. 1830 H street.

1888. TITTMANN, O. H. (Otto Hilgard),
Coast and Geodetic Survey. 1019 Twentieth street.

1878. TODD, DAVID P. (David Peck),
Amherst College Observatory, Amherst, Massachusetts.

1873. TONER, J. M. (Joseph Meredith),
1445 Massachusetts avenue.

1886. TRENHOLM, WM. L. (William Lee),
160 Broadway, New York, N. Y.

1890. TRUE, A. C. (Alfred Charles),
Department of Agriculture. 1604 Seventeenth street.

1882. TRUE, FREDERICK W. (Frederick William),
National Museum. 1322 Yale street.

1882. UPTON, WM. W. (William Wirt),
Atlantic Building, 930 F street. 1746 M street.

1880. UPTON, WINSLOW,
Brown University, Providence, Rhode Island.

1890. VAN HISE, C. R. (Charles Richard),
University of Wisconsin, Madison, Wisconsin.

1892. WADSWORTH, F. L. O. (Frank Lawton Olcott),
University of Chicago, Chicago, Illinois.

1883. WALCOTT, C. D. (Charles Doolittle),
Geological Survey. 1746 Q street.

1881. WALDO, FRANK,
95 Mercer street, Princeton, New Jersey.

1872. WALKER, F. A. (Francis Amasa),
Massachusetts Institute of Technology, Boston, Mass.

1876. WARD, LESTER F. (Lester Frank),
National Museum. 1464 Rhode Island avenue.

1889. WATKINS, J. E. (John Elfreth),
National Museum. 1626 S street.

1894. WEAD, C. K. (Charles Kasson),
Patent Office. 1345 Q street.

1882. WEBSTER, ALBERT L. (Albert Lowry),
107 Drexel Bldg., cor. Wall and Broad sts., New York.

1876. WHITE, C. A. (Charles Abiathar),
National Museum. 312 Maple avenue, Le Droit Park.

1884. WHITE, DR. C. H. (Charles Henry), U. S. N.,
Brooklyn, New York.

1885. WILLIS, BAILEY,
Geological Survey. 1004 Twenty-second street.

1887. WILSON, H. C. (Herbert Couper),
Carleton College Observatory, Northfield, Minnesota.

1880. WINLOCK, W. C. (William Crawford),
Smithsonian Institution. 2005 O street.

1891. WINSTON, ISAAC,
Coast and Geodetic Survey. 1325 Corcoran street.

1875. WOOD, JOSEPH,
1003 Pennsylvania avenue, Pittsburg, Pennsylvania.

1871. WOOD, LT. W. M. (William Maxwell), U. S. N.,
Navy Department.

1895. WOODS, W. E. (William E——),
Architect's Office, Capitol.

1883. WOODWARD, R. S. (Robert Simpson),
Columbia College, New York, N. Y.

1885. WRIGHT, GEO. M. (George Mitchell),
Akron, Ohio.

1887. WÜRDEMANN, H. V. (Harry Vanderbilt),
805 Grand avenue, Milwaukee, Wisconsin.

1874. Yarrow, H. C. (Harry Crécy),
814 Seventeenth street.

1884. Yeates, W. S. (William Smith),
State Capitol, Atlanta, Georgia.

1885. Ziwet, Alexander,
University of Michigan, Ann Arbor, Michigan.

Whole number, 214.

ON THE UNCERTAINTY OF CONCLUSIONS.

BY

T. C. MENDENHALL.

ADDRESS AS RETIRING PRESIDENT.

Delivered January 20, 1892.

About seven years ago, on the morning of a cold day in winter, a rough-looking, scantily dressed man was observed to leave a freight car, which was standing upon a side-track near a small country town, and make his way rapidly into the fields and woods beyond.

From his appearance it was evident that he belonged to that vast army of tramps which is never in need of mobilization and which carries upon its muster-rolls many who possess most of the virtues of the good and none of the vices of the bad, having lost only the power of further resistance against continued antagonism and unfriendly environment.

The behavior of this man excited no comment, and his existence was remembered a few hours later only because of the discovery of the body of a stranger, who had evidently been murdered, on the floor of the car which he had been seen to leave. Pursuit followed immediately, and capture within a day or two. One or two clever detectives interested themselves in finding evidence of his guilt, and within a few days had prepared a case which lacked little in the detail of its elaboration or in its artistic finish.

It was proved that two strangers were seen in the suburbs of the town at a late hour on the previous night, although they were not together. The prisoner was identified beyond doubt as the man who hastily left the car in the morning.

The murderer had left no means of identification except a small piece of muslin, evidently torn from the sleeve of his shirt, and which was stained with the blood of his victim. On the arrest of the prisoner one or two blood stains were found upon his clothing, and, what was more convincing than all else, the bit of sleeve found in the car fitted exactly into the place in his own garment, from which it must have been torn in the struggle which preceded the crime.

While all of this evidence might be classified as "circumstantial," it was so complete and satisfactory that no jury could be expected to entertain serious doubt as to the guilt of the prisoner, and, in spite of his protestations of innocence, a sentence to life imprisonment was in accord with the judgment of the general public.

Only a few weeks since this man was set free and declared to be innocent of the crime for which he had already served seven years at hard labor, the misleading character of the evidence on which he was convicted having been exposed through the voluntary confession of the real criminal. The facts thus brought out were, briefly, as follows:

There were three men in the case. The first, who was afterward murdered, slept upon the floor of the car when the second, the real murderer, entered it. In the dark he stumbled over the sleeping man, who awoke and immediately attacked him. The quarrel did not last long, the original occupant being left dead upon the floor of the car while the murderer quickly made his escape, leaving the village and neighborhood behind him as far and as fast as possible. An hour or two later the third man, seeking shelter and sleep, finds his way into the car, and dropping on the floor, is soon in a deep slumber. He awakes at break of day to find that a dead man has been his companion, and to see that his own sleeve is smeared with the blood of the victim. Alarmed by this discovery, and realizing in some degree the perilous position in which he is thus placed, he tears off the stained portion of his garment, and, hastily leaving the car, he flees from the scene as rapidly as possible.

Nothing can be more simple or more satisfactory than this account of the affair, and yet nothing is more natural than that he should be accused of the crime and brought to trial. The evidence against him was convincing and it was all absolutely true. It was not strange, therefore, that his conviction and imprisonment should follow.

It will doubtless appear to many that the foregoing is too closely allied to the sensational to serve fitly as an introduction to an address prepared for a society of philosophers, and I am ready to acknowledge the apparent validity of the criticism. I am led to its selection, however, because it is an account of an actual occurrence, which illustrates in a manner not to be misunderstood a not unrecognized proposition to a brief exposition and partial development of which I ask your attention this evening. This proposition is that *in the treatment of many questions with which we are confronted in this world our premises may be absolutely true and our logical processes apparently unassailable and yet our conclusions very much in error.*

No department of human knowledge or region of mental activity will fail to yield ample illustration and proof of this proposition. An astonishingly large number of debatable questions present themselves to the human intellect. Many of them are conceded to be of such a nature that differences of opinion concerning them must continue, perhaps, indefinitely.

But there is a very large and a very important class of problems, the solution of which is apparently not impossible and often seemingly easy, regarding which the most diverse views are most persistently held by persons not differing greatly in intelligence or intellectual training.

Men whose business is to weigh evidence and to reach correct conclusions in spite of inadequacy of information and perversion of logic constitute no exception to this statement, but, on the contrary, furnish many of its most notable illustrations.

Many of the questions which present themselves to our

jurists and juries are simply questions of fact, and the testimony on which the determination of such questions depends often comes from persons who are neither interested nor dishonest. In such cases it ought to be easy to reach a true conclusion, but there is often failure, growing out of honest differences of opinion.

An eminent attorney not long since referred in conversation to a certain decision of the Supreme Court of the United States concerning which there had been a strong dissenting minority. The question was one which involved neither passion nor politics, and he declared that to him it seemed utterly impossible for a disciplined mind to reach other than one conclusion regarding it.

In any review of this subject such as is here suggested it is neither necessary nor proper to refer to the numerous instances of utter failure in our judicial system attributable to a lack of integrity on the part of those who administer the laws or to the mischievous results of appeals to passion or prejudice by unprincipled advocates. It is sufficient to recognize the fact that failure in the administration of law is not uncommon where witnesses are honest, juries intelligent and well-meaning, and judges incorruptible.

The rapidly increasing number of controversies within the church, to say nothing of those in which the disputants are on opposite sides of the wall, show conclusively that the logic of the theologian must sometimes go at a limping gait. In political or social economy there is great diversity of opinion among good and able men. Certain financial legislation by Congress is honestly thought by many people to be necessary to prevent wide-spread disaster and the financial ruin of one of the largest and most important classes of our citizens; by other equally intelligent and equally honest men such action on the part of the National Legislature is condemned as dishonest in principle and sure to be fatal to the business interests of the country.

A large number of able and patriotic men address themselves to the solution of the problem of the adjustment of

duties upon imported merchandise. All have access to the same store of experience; the discussions and investigations of the past are open to all alike. In the end, however, their conclusions, even as to elementary principles, are diametrically opposed to each other.

But I have neither the time nor the disposition to enter into an exhaustive examination into the miscarriage of logic in the regions of politics, religion, or social science. I must restrict myself to some consideration of the uncertainty of conclusions reached by what may be broadly included under the general term "the exact sciences," a division of the subject not unlikely, I hope, to be of some interest to members of this Society.

At the threshold of the investigation we are confronted by the term "*exact sciences,*" and it is of the utmost importance to reach a clear understanding of the meaning of this phrase in the beginning. By some writers its application is limited to the mathematical sciences or substantially to pure mathematics. This does not seem, however, to be in accord with the general usage among scientific men, and a wider significance will be here given to it.

Pure mathematics may, and posssibly must, be regarded as a mode of thought; as symbolic logic; as an abridgment of mental processes by the selection of that which is common to all, and its formal expression by means of signs and symbols. Intellectual operations which, on account of their complexity and length, would be possible only to a few of the highest capacity are by the aid of mathematics brought within the range of the many. In virtue of the simple and beautiful nomenclature of the science, one can see at a glance, in a formula or equation, the various relations, primary and secondary, direct and implied, which exist among the several magnitudes involved, which, if expressed or defined in ordinary language, would be beyond the understanding of most intelligent people.

The principles and rules governing mathematical operations have been, in the main, so well worked out and so uni-

versally agreed upon that in mathematics one can hardly go astray, at least not without the certainty of almost immediate detection and conviction at the hands of many skilled in the use of this wonderful intellectual device. When dealing with quantity in the abstract, or with matter under just such restrictions or possessed of just such properties as are prescribed, mathematics becomes a machine of certain performance, the output of which can only be in error through the conscious or unconscious mistakes of the operator. As such it challenges the admiration of all, and it must forever be regarded as among the first, if not, indeed, the very first, of the few really splendid creations of the human intellect. When Plato, in reply to a question as to the occupation of the Deity, answered, "He geometrizes continually," he emphasized the dignity and the incontrovertibility of mathematical reasoning.

It is no reflection, then, upon the importance and value of the science of mathematics to leave it upon the pedestal which it rightfully occupies, considering it as separate and apart from other sciences. In their development it may and does play a most important part, in which, however, it is identified rather with the investigator than with the subject investigated; for, in studying the elementary principles of abstract dynamics, one may follow the now somewhat antiquated and cumbersome processes of Newton or the more simple and elegant methods of Clifford or Maxwell, but the results will in all cases be the same.

Before finally dismissing the pure mathematics, however, especial attention must be invited to one or two principles involved in their application by way of contrast with the condition of things which exists in the domain of the other sciences. It is sometimes declared by way of a criticism of mathematics that "what comes out of it is never better than what goes in." In a certain narrow sense this is true, but in a broader and truer sense it is as false as it would be to say that grain and fruit are no better than the soil from which they spring.

The mathematician has the great advantage over the physicist, the chemist, or the geologist that he not only can, but almost necessarily must, completely define the elements with which he has to deal. If he deals with matter, before he can put it into his equations he must needs restrict it as to form and dimensions and endow it with definite physical properties, the relations of which are capable of analytical expression. If, after this, his power of analysis is sufficiently great, the conclusions which he reaches can have no element of uncertainty in them, provided always they are considered as referring only to the supposititious material with which the investigation was begun. That the conclusions are not in harmony with known phenomena is evidence only of the fact that the material of nature is not the material which is symbolized in the formula, and that certain properties which are common to both are modified in the former by the presence of others which are not attributed to the latter. When MacCullagh, Neuman, Stokes, Sir William Thomson, or Maxwell each evolves a dynamical or mechanical theory of light, a lack of agreement among them or with known principles of optics can generally be traced to the fact that the medium in which they suppose the action to take place has not been endowed with the same common properties by all, and that in every case it falls short of an exact representation of the real ether itself. With this important restriction upon mathematical reasoning kept continually in mind, mathematics may be safely set aside as the "one science of precision."

What, now, are the characteristics of the so-called "exact sciences" other than pure mathematics? Without attempting a rigorous definition or a precise classification, it is sufficient for the purpose at hand to declare that the exact sciences are those whose conclusions are capable of being, and for the most part are, established by experiment and verified prediction.

Among these exact sciences the most notable, in degree of exactness, is the science of astronomy. Although the conclusions reached in the study of astronomy may not in gen-

eral be established by experiment, the marvelous accuracy with which its predictions are verified has long ago placed it far in advance of other sciences. An inquiry into the cause of this excellence will not show that the logic of the astronomer is any more rigorous than that of many others engaged in scientific research, but rather that the premises on which he reasons are simpler, and, what is of greater importance, more nearly sufficient. Until a very recent period in its history, astronomy, although dealing with matter, has been concerned almost entirely with only one of its many properties. The one property thus far assumed to be common to all matter is that long known but still mysterious attraction in virtue of which there exists a stress between every particle and every other particle in the universe, according to a law the discovery and exposition of which justly entitles Newton to be considered the greatest philosopher of all ages. It happens that the hundreds and possibly thousands of other properties possessed by or inherent in matter have little if any influence on the dynamics of masses widely separated from each other, and therefore a knowledge of the law of gravitation seems to be sufficient to enable the astronomer, having, of course, obtained the necessary data from observation, to trace the paths of the planets and to foretell the configuration of the heavens many years in advance. Within the past twenty-five years, however, the splendid discovery of spectroscopy, aided by great improvements in photography, has given rise to a new astronomy, known as physical, as distinguished from gravitational astronomy. The new science deals with a matter of many properties, some of which are but little understood. While its conclusions are of vital importance and of intense interest, they result from deductions in which the premises are insufficient and are proportionately uncertain. The new astronomy must for a long time abound in contradictions and controversies until, and largely through its development, we shall possess a knowledge of the properties of matter when subjected to conditions differing enormously from those with

which we are now quite familiar. Because one astronomer declares that the temperature of the Sun is 20,000° F., and another, equally honest and capable, says it is not less than 20,000,000° F., it must not be inferred and it never is, except by the superficial, that the whole science of solar energy is a tissue of falsehoods, and that those engaged in its development are deliberately planning an imposition upon the general public. Even such widely varying results as these may be based on observations that are entirely correct and experiments that are beyond criticism. The discussion of the results obtained by observation and experiment may follow, in both cases, the very best models and yet the conclusions may be erroneous and contradictory, owing to the insufficiency of data in the beginning.

Unfortunately the omission of one or more important quantities from the equations of condition is not always known or suspected. The older, more exact astronomy is occasionally caught tripping in this way. An interesting example of recent occurrence is to be found in certain observations for stellar parallax made a few years ago by members of our own Society. The observations were long continued, the instruments used were of a high character, and the observers were skillful. These conditions unquestionably promise success. It was something of a surprise, therefore, when a reduction of the observations gave for the parallax a negative result. As such a result could in no way be possible, except, perhaps, through the assistance and intervention of a curvature in space (in virtue of which if a man's vision was not limited he would, by looking straight forward, see the back of his own head), it was assumed that the work was not as well done as it seemed to be, or that some imperfection in the instrumental appliances had been overlooked. It now appears, however, that this record may be reöpened, and that the results may prove to be of as great value as originally anticipated. Researches carried on during the past year or two have with little doubt established the fact that the latitude of a point on the earth's surface is

not a fixed quantity, but that on the contrary it varies through a small range during a period somewhat greater than a year. It is believed that if this hitherto unsuspected variation be applied to the parallax observations referred to above, the seeming absurdity of the result will vanish.

If astronomy, the foremost of the exact sciences, is not free from the fault of basing conclusions upon insufficient premises, it will not be expected that among other sciences the evil will be of less magnitude.

When we consider the sciences of heat, light, electricity, magnetism, and other specially investigated properties of matter, all of which are usually included under the general head of "Physics," we meet with a formidable rival of astronomy in the extent to which they are entitled to be considered as exact sciences.

Physics treats of all the properties of matter, not omitting that which is the special domain of astronomy. As if this were not enough, the demands upon the science are such that it must also deal with that which is not matter or, at least, is not matter in the ordinarily accepted sense. Although physics deals with all of the properties of matter, no physicist knows them or, possibly, half of them. Perhaps not one of them is entirely and completely known. It would seem, therefore, that this science must of necessity be one of uncertain conclusions. That it is far from deserving so sweeping a criticism is due to the fact that the properties of matter are not so closely interrelated as to make it impossible to isolate one or more of them in experiment, and thus the problem is vastly simplified. It is probably impossible to do this rigorously in any case, so that there must always remain a small residuum of uncertainty due to the interference of unknown or imperfectly understood properties of matter.

Thus it is possible to treat a mass of matter as though it possessed mass only, ignoring its electrical, magnetic, or optical properties, its relation to heat, its elasticity, and other physical characteristics, and investigate its behavior under the law of gravitation alone; its optical properties may be

found to be nearly independent of its relation to heat, electricity, magnetism, etc., and so, in turn, each characteristic may be studied alone and equations are obtained in which the number of constants is comparatively small. It is only after this plan has been pretty thoroughly worked out that it becomes possible to investigate the interrelations of these various properties, which are often obscure and difficult of detection. Their discovery, however, especially one or two great generalizations pertaining to them, such as that of the conservation of energy, must be regarded as the grandest triumph of physical science.

The science of physics is that which is most drawn upon in the formation of the so-called applied sciences. Wedded to mathematics as it is (and no amount of personal abuse on either side can ever furnish good reason for divorce), it becomes the mother of engineering in all of its various forms, Through and by it the forces of nature have been directed· the elements have been subdued and some of them overcome, and man has made himself master of the world. Its marvelous progress has, therefore, been observed by the people and is understood by them perhaps to a greater degree than that of any other science. The most eloquent orators and the ablest writers have employed their genius in sounding its praises.

It is not too much to say that when intelligent people speak, in a general way, of the wonderful things which science has accomplished during the past half century, they have in mind, for the most part, the applications which have been made of discoveries in physical science. I think no one can justly question the assertion that of the several causes which have produced the splendid advances in the material interests of the whole world during the nineteenth century, science has contributed far more liberally than all others. So remarkable have been her achievements that all the people have come to look upon her as being nearly if not quite infallible. A reputation of which the votaries of science may be proud has been established, but, at the same time, one

difficult to maintain. Here, as elsewhere, it is a good name only that is worth counterfeiting. It is quite worth the while of one devoted to the interests of pure science alone to occasionally inquire whether an impure article is not being placed upon the market. However indifferent he may be to the welfare of the general public, his own selfish instincts should incline him to such a course. He cannot clear his own skirts by declaring that the public deserves to be humbugged if it permits itself to be, for in this, as in everything else, the counterfeit when successful is not readily detected, and it is often made to appear more attractive than the genuine article.

In respect to this matter physical science presents two aspects. In a large degree it is a science of certain conclusions, and any false deduction is readily exposed by means of the many severe tests to which it may be subjected. On the other hand, in some of its branches it has not yet been found possible to isolate the elements which form a rather complex whole, and it therefore remains an observational rather than an experimental science. In the latter aspect it becomes comparatively easy prey for charlatans and well-meaning but ignorant non-professionals.

In no department of physical science is this better illustrated than in meteorology, the oldest and most abused of all sciences. From its early days, when weather forecasts were expressed in simple rhyme, to the present, when they are issued in a prose which in its scope and richness of vocabulary sometimes excites our highest admiration, meteorology has been a favorite victim of pretenders, conscious and unconscious. For years the people, after having first believed in, have patiently borne with the predictors of disaster in the form of abnormal meteorological disturbances. They have suffered great mental distress and they have lost enormous sums of money on account of floods, tidal waves, and earthquakes which never came, rains that never fell, and winds that never blew. They were becoming accustomed to this sort of thing and were beginning to understand the

spirit which guided the real meteorologists as manifested in the efforts of the great Weather Bureaus of the world, our own among the first, to foretell with a good degree of certainty what might happen within the next twenty or thirty hours. But not many months ago they were again brought to a high pitch of meteorological excitement by the somewhat sudden and certainly unexpected appearance of the "Cloud-compelling Jove." He came not in the singular, but in the plural, and each of him brought the best and most scientific device for producing a rainfall whenever and wherever a sufficient thirst was found to exist. The history of this new industry cannot yet be written. It is still in its infancy. The fallacy of its methods has already been commented upon in a public journal, by a distinguished member of our own Society, but a few remarks upon its somewhat meteoric career during the past season will not be out of place in connection with the subject now under consideration.

The columns of the daily press reflected the general interest which was felt in the matter, especially in parts of the country where rainfall was greatly needed. As is always the case under such circumstances, the strong and entirely natural desire that its artificial production might be accomplished was soon converted into a belief that it had been, and a readiness to accept the flimsiest sort of evidence of relation between the means employed and the end sought. This confidence materialized, or better, perhaps, was taken advantage of in the formation of an "Interstate Artificial Rain Company, Limited" (I am quoting from the daily papers of November 10, 1891), which, after the manner of its kind, was apparently organized not for the purpose of actually producing rain, but for the formation of other joint stock companies ready to purchase the secret method of doing it. An alleged experiment, on which a business transaction was based, is thus described:

"The party arrived in the city on Sunday, November 1, and commenced operations on Sunday evening in a small outhouse on the edge of town. The conditions were ex-

tremely unfavorable for rain. No results could be seen at first, but on Friday the sky became overcast with clouds. On Saturday a high south wind prevailed, and on Saturday night some rain came from the southwest. On Sunday rain fell all day, and at night a norther arose. Reports from 100 to 150 miles around this town show that rain fell on Sunday in most localities in considerable quantities." So convincing was this to the buying company that the secret process was purchased by them for the sum of $50,000, "after which," the account rather unnecessarily adds, the selling company "left for home." But a business so profitable as this was not to be long without competition, and a few weeks later a telegram is sent to the leading newspapers of the country, announcing that a professor in a Western State (it is pleasant to note that most of these public benefactors are "professors") is prepared to furnish rain more promptly and at less cost than the genius whose machinery and methods have invited public approval. Proposals to do the county sprinkling at so much per acre are invited and offered, and at one time it seemed as if the whole business would be ruined by over-production.

One of the most interesting phases of this subject was the attitude in reference to it assumed by a large part, possibly the greater part, of the intelligent public. It was one of expectancy and limited confidence. "Why not?" was commonly asked. "Look at what science has done within the last twenty-five years. Can anything be more astonishing; and is the artificial production of rainfall more difficult and more wonderful than many things which are now commonplace?" To many the logic of the experiments was convincing. After many battles rain had fallen, long lists of examples have been prepared, and hence it must be possible to produce rainfall by cannonading. If these views were entertained by a considerable number of intelligent people, and it is believed that they were, the situation is one which ought to be full of interest to men of science, involving, as it does, both a tribute and a warning.

It would be good for all if the intelligent public was in the habit of looking a little more below the surface of things. It is too much in the way of assuming that the president of the company engaged in exploiting an important invention or device is the genius who first discovered the principle in virtue of which it operates. It loses sight of—no, it does not lose sight of, because it never knew—the patient toil, the unselfish devotion, and, what is perhaps more important, the unflinching honesty with which a few men of the highest intellectual capacity have from the earliest times given themselves to the study of the laws of nature.

It would surprise the public to know how long ago and by whom many of the most recent and most brilliant applications of science *were made possible.* Would it not be in the interest of all if men of science were more ready and willing to take the intelligent public into their confidence; and would not the public, if familiar with the history of scientific investigation and accustomed to scientific modes of thought and criticism, be less the prey of charlatans and well-meaning but ill-informed enthusiasts? A better knowledge on both sides would lead to a better appreciation of both sides, and the real worker in science would seldom go without that public recognition which has too often been denied to the ablest men. No better illustration of this can be found than in the life of the distinguished first president of this Society, to stand in whose place must always be an honor to any man. With his great work as Secretary of the Smithsonian Institution the public is fairly well acquainted, and it has not been backward in bestowing honors in recognition of that work. Unfortunately, comparatively few know of what must be regarded, I think, as his greater work, the original researches in which he was engaged and in which he was so singularly successful, before he became identified with the Institution to which he gave the greater part of his life. Scant justice has yet been done to this important part of a career which must always be an inspiration to members of this Society.

But I am warned that the brief time during which I can claim your attention tonight is quite insufficient for anything like a full exposition of the theme which I have selected, and I must, I fear, somewhat abruptly turn about in order that I may leave with you in somewhat more definite language one or two thoughts which I have attempted to develop by illustration and example.

Recurring to the unfortunate victim of circumstantial evidence whose experience was related in the beginning, it will be admitted that the judge who charged, the jury who convicted, the witnesses who told the truth, and the approving public were all in error, in that they failed to recognize that *there was another way of explaining what had happened.* It does not necessarily follow that the explanation which explains is the true one. There are many natural phenomena which are in entire accord with more than one hypothesis. Indeed, there are some things which may be perfectly accounted for on an infinite number of suppositions, but it does not follow that all or any one of them must be accepted. There is nothing especially novel in this proposition, but I submit that to a failure to keep it in sight must be attributed a large measure of the uncertainty of the exact sciences, as well as much useless and bitter controversy in science, religion, ethics, and politics.

As a sort of corollary to this proposition I suggest that many reasoning and reasonable people are indifferent to, if not ignorant of, the fact that the value of evidence is greatly dependent on *the way in which it arranges itself.* To many this may be made a little clearer if I borrow a phrase from one of the most exact of modern sciences and speak of evidence as presenting itself *in series* or *in parallel.* Without pushing the analogy further, the superior strength of the latter arrangement will be evident upon reflection. On another occasion, I have referred at some length to the numerical representation of the value of testimony and to some conclusions which are easily reached. As bearing upon the

subject in hand, a single example of this method of treatment may be useful:

Let there be two witnesses, A and B. Suppose that A tells the truth 51 times out of 100—that is to say, assume that honesty holds the controlling share in his stock of moral principles. Let B be equally truthful and no more. Then if these two testify independently to the occurrence of a certain phenomenon it is more likely to have occurred than if either one alone bore witness. This is evidence in parallel. If, however, A testifies that B declares that the thing happened, it is less probable than if based on the testimony of either alone. This is evidence in series. Put as boldly as this, no one doubts the higher value of the first arrangement; but it is believed that a more careful consideration of this distinction will do much to secure a better judgment, not only where human testimony is involved, for here it has long been an established principle, but where conclusions are based on observation and experiment.

It is of the utmost importance, therefore, that men of science, before accepting a theory or a hypothesis as final, should carefully scrutinize the steps by which it has been established to see that they are not only sufficient but necessary. The true philosopher will be slow to claim that the theory which he finds sufficient to explain all of a given class of facts is the necessary and true one; he will be constantly on the lookout for a new fact which his theory will not quite explain, and he will have much consideration for his friendly competitor who finds a different hypothesis equally satisfactory and efficient. Above all, he will not pride himself on the steadfastness of his views and will rarely bind himself to be of the same opinion this year as last.

If the general public could be made to understand the limitations by which science is circumscribed, the tentative and ever progressive character of scientific investigation, it would be good for the public and good for science.

The human race is greatly handicapped by the presence of a good number of people who strenuously object to being

disturbed. During a decade, generation, or century these good but sometimes unpleasant people plant themselves along certain lines in the domain of science or politics or religion proclaiming essentially that " here and here only is the truth, and here we fix ourselves forever." After a while they somewhat unwillingly and with no very good grace move forward into a new position, again honestly affirming and believing that the end has been reached. A better knowledge and a broader human sympathy would reveal to them the hitherto unsuspected fact that truth may at the same time be *here* and *there.*

In the dissemination of this knowledge and the cultivation of this sympathy, science should lead, not follow. No scientific organization so young in years has done more along these lines, especially by reason of its extensive membership and the vigor and enthusiasm of its branches, than the Society over whose deliberations during the past year I have been permitted to preside.

For the honor thus bestowed I beg now to make my formal and grateful acknowledgments.

THE RELATION OF PHILOSOPHY TO PSYCHOLOGY AND TO PHYSIOLOGY.

BY

JOSEPH LE CONTE.

[Read before the Society, January 30, 1892.]

DEFINITION OF PHILOSOPHY AS USED IN THIS ARTICLE.

I am embarrassed at the outset by the different meanings given to the word philosophy. Sometimes it is used to signify the mere attitude of the mind toward truth, and sometimes it refers to the subject-matter of thought. Again, on this latter and more usual view, sometimes it refers to the highest aspects of knowledge in each department, and especially to the relation of these highest truths to one another, and thus becomes the science of the sciences. Sometimes it refers to the study of the grounds of validity of human knowledge, and sometimes, lastly, it refers to the study of the phenomena of the self-determined activities of free spirit as contrasted with the necessary activities of nature—*i. e.*, metaphysics as contrasted with physics, or metakinetics as contrasted with kinetics. It is somewhat in this last sense—*i. e.*, as the science which treats of the activities of free, self-conscious spirit (Jouet) or the phenomenology of spirit (Hegel)—that I shall use the term philosophy in this article. There certainly ought to be a name for the study of these very distinct phenomena. I am encoûraged to use this one by its use by Jouet in the same or a similar sense; but if any one can suggest a better one I will gladly adopt it.

RELATION OF MAN TO ANIMALS.

If my view of the mode of origin of man's spirit be true; * if spirit in embryo in animals, developing through all geological times, came to birth and capacity of independent life, and therefore immortality in man; if man alone lives in a new and higher spiritual, immortal world wholly unknown to animals; if the difference between the animal world and the distinctive human world—the phenomena of animal life and the distinctive phenomena of human life—be indeed so great, not only in degree, but also in kind,† as my view would make it, then surely the phenomena of this higher world constitutes not only a distinct department of thought, but by far the most distinct of all; but in these latter days it has become the fashion to minimize the distinction between man and animals, and therefore to assimilate, or even to identify, psychology with physiology on the one hand and with philosophy on the other. There can be no doubt that the limits of these three departments are very imperfectly defined in the minds of even the best thinkers. For some years I have striven to make them clear in my own mind. My object in this article is, if possible, to make them clear in yours.

In an article published in the Princeton Review for May,

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* See the author's book, "Evolution and its Relation to Religious Thought," part III, chapter IV.

† Many will object to this statement. It is the fashion now to say that the psychical difference between man and animals is a difference in degree only and not in kind. I wish some one would define the characteristics of these two kinds of differences. In pre-Darwinian times the distinction was plain enough. Differences of degree came in a natural way; differences in kind in a supernatural way. But now that evolution is established, it is evident that all kinds of differences come in the same way—*i. e.*, by a natural process of evolution. Either, therefore, there is no such thing as difference in kind at all or else it is only a greater degree of difference in degree. We call it difference in kind when the difference is very great or came about somewhat suddenly or else where it is without gradation or gradations are lost. If any difference in nature deserves the name of difference in kind, surely it is that between man and animals.

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1884, entitled "The Psychical Relation of Man to Animals," I tried to show in what consists the essential difference between man and animals in those psychical phenomena in which they seem to approach most nearly: (1) In speech, between the imitative speech of parrots and very young children and the rational speech of man, which in fact commences only at about three years of age. (2) In art, between the empirical constructive art of animals and the rational, indefinitely progressive art of man. (3) In fine art, between the so-called music of birds and the real music characteristic of man. (4) In the realm of thought, between the intelligence of animals and the reason of man—the precepts of the one always associated with things and the concepts of the other abstracted from things. (5) In the imagination of animals (*e. g.*, in dreams), reproducing only past experiences, and the creative imagination of man, using the materials of experience, but combining them so as to make wholly original pictures. (6) In the consciousness of animals—*i. e.*, consciousness of the external world, and the self-consciousness, or consciousness of the internal world of ideas, characteristic of man. (7) In the volition of animals, determined by external conditions, and the self-determined will, the free moral choice, and therefore moral responsibility, characteristic of man. I tried to show that the differences in all these are of similar order, and all are the necessary result of and completely explained by my view of the origin of spirit by gradual evolution through the animal series and its birth into a higher world in man.

ILLUSTRATED BY MEMORY.

In order to explain more fully what I mean, and also as a fitting introduction to the subject now in hand, I will take one more example, viz., that of memory. I take this because there is no faculty of mind more fundamentally important, and yet none which seems at first sight more absolutely identical in man and in animals. Now, I believe that animals have no memory at all in the sense in which we know

it in ourselves. This may seem a very paradoxical statement. It will be at once objected that animals seem to have memory; they behave exactly as if they had; they profit by experience; learn habits; are capable of education, &c.; all of which seems impossible without memory. What, then, is the difference between such memory and what we know as memory in ourselves? It is this: The events remembered by animals are not appreciated as past events in the history of self, of the *ego*. There is no consciousness of self as abstracted from conscious phenomena, and no conception of time as abstracted from events, and therefore no conception of events as occurring in the history of the *ego*. An impression, pleasing or painful, is made on the nerve-terminals, is transmitted to the brain, determines molecular changes there, which in their turn determine psychical changes in consciousness, emotions, desires, &c., and these again determine corresponding actions. Now, whenever the sense-impression is repeated the corresponding feelings are reproduced and appropriate actions follow. All these phenomena, physical and psychical, are so closely united that the whole may be regarded as in some sense automatic. In the lowest animals and in sensitive plants the automatism is evident and admitted by all; but in higher animals, partly because the phenomena are really higher and partly because we inject our own self-consciousness into them, it seems very like what takes place in ourselves; but in fact there is a wide difference. In man we have exactly the same interconnected series, but in addition, and on account of the idea of self as abstracted from the phenomena of consciousness and of time as abstracted from events, these changes are transformed into an idea of an event occurring in the history of the *ego*.

PROVED BY HISTORY OF THE INDIVIDUAL.

If we need any proof of this distinction we find it in our own individual history. The child a year old, or even a few months old, seems also to the outside observer to have memory. It profits by experience, learns habits, good or

bad, is capable of education, and yet, although habits then acquired may continue to influence conduct throughout life, we know by introspection that memory as a history of self does not reach so far back. There was as yet no consciousness of self, no concept of time, nor of events occurring in time or constituting a history. The child's life was strictly animal. We could never know this by observation of children, but only by introspection of ourselves.

Thus there are two kinds of memory (if we must call it so) as there two kinds of consciousness, will, thought, imagination, indeed of every faculty of the mind, which are related to each other as shadow and substance, or better, as embryo and child. The one is a change in the brain; the other a change in the *ego*. As there are two kinds of consciousness, viz., animal consciousness, or consciousness of external phenomena, and self-consciousness, or consciousness of internal phenomena, so there are two kinds of memory, viz., animal memory, or a reviving of past feelings by repetition of external conditions, and self-memory, or memory as a history of the *ego*.*

FULLER EXPLANATION.

This point is so fundamental that I must stop to explain if possible more fully. In any case of animal action there are a number of changes in connected series: 1. Impression on a nerve-terminal; 2. Transmission centripetally along a sensory fiber; 3. Changes in a sensory brain-cell; 4. Transmission by a connecting fiber to a motor brain-cell; 5. Changes in the motor brain-cell; 6. Transmission centrifugally along a motor fiber; 7. Muscular contraction determining changes in the external world. This is all. It is true

* There are in fact three or four degrees of memory, although only those above mentioned are usually called memory: (1) Unconscious or organic memory, such as the tendencies of growth in the embryo, inherited from ancestral history. (2) Conscious memory of animals. There are two degrees of this: (*a*) instincts or inherited habits; (*b*) acquired habits, the result of individual experience. (3) Self-conscious memory, or history of the *ego*.

that in the higher animals, and more and more as we go up, connected with Nos. 3 and 5—*i. e.*, changes in brain-cells, sensory and motor—there seems to be consciousness and will, but it is certain that the animal is not conscious of the consciousness nor of the will. The whole series is determined absolutely by the external impression. It is simply necessary response to external stimulus; but in man, in addition to all this, there is a whole world of phenomena entirely peculiar to him. In man external impression and brain changes excite or determine concepts or ideas abstracted from things, and these in their turn determine other ideas of higher orders, and therefore theories, philosophies, sciences, religions, &c. Once started, and there is no end of combinations having no immediate connection with the external world. Again, in animals the initiative of the whole series is external, and the internal response and the completion of the cycle of changes is inevitable. In man alone there seems to be a self-active agent within, which may choose to respond or may not. It may choose rather to operate within its own interior world, using materials either gathered from without or furnished from within. It may also, without any external impression, by motives generated among its own phenomena, internal impressions, freely determine motions and changes in the external world.

THE TWO WORLDS.

Thus there are two worlds—an external and an internal; a material and a spiritual. The animal body is an exquisitely adjusted instrument of communication between. The key-boards are nerve-terminals on one hand, in contact with the material world, and brain-cells on the other, in contact with the spiritual world. The material world plays on the one and determines changes in spirit; spirit plays on the other and determines changes in the material world. In animals spirit is in embryo—asleep or, at most, dreaming—or even, perhaps, somnambulistic; unconscious of self, and responds reflexly as sleepers do, and therefore immediately

and definitely back on the external world only. In man spirit is wide awake and conscious of self. It deliberates whether to respond or not and how it will respond. It may decide (1) not to respond at all, or (2) to respond, not by determining external changes, but by free activities within its own interior world; or (3) it may act freely within its own sphere without external stimulus; or (4) it may freely initiate changes in the external world without any stimulus from without.

Thus plants are unconscious of any world, animals are conscious of the external world only. Man is conscious of the external world like animals, but also of the internal, spiritual world; or again, in animals physiological changes are the cause of psychical phenomena. In man, very often, only the same, but often also, in addition, psychic changes are the cause of physical phenomena. In animals the whole series begins and terminates externally. In man it may begin either externally or internally. In God it always begins internally, for all natural phenomena have their cause in the Divine mind. Thus man partakes both of the animal and of the Divine nature.

SUBJECT-MATTER OF PHILOSOPHY LIMITED TO MAN.

Now, philosophy, according to my view, commences right here, in this highest order of phenomena. It is the science of the *ego*, the science of awakened spirit—of free, active spirit. All else that is usually treated by mental philosophers belongs for the present to psychology, but may, perhaps, eventually be claimed by physiology; for it is, at least, more allied to this department than it is to what I have called philosophy. If, for the present, we make here three departments—as, for the present, I suppose we must—then all the realm of mere life belongs to physiology, all the realm of conscious life to psychology, and all the realm of self-conscious life to philosophy; or, to put it another way, all the phenomena determined by mere life-force belongs to physiology, all the phenomena determined by that higher

form of life-force, which may be called nerve force or psychic force, to psychology, while all the phenomena determined by free, self-conscious spirit is reserved for philosophy. If so, then philosophy belongs to man alone, and, in man, deals only with distinctive human phenomena.

But how is it now, especially in the English and Scotch schools of philosophy and still more especially among modern philosophical physiologists and psycho-physicists? The line between the two widely separated, extreme departments, physiology and philosophy, is drawn somewhere in the region of psychology—some putting it higher, some lower, and some claiming the whole of psychology for the one or the other. There is therefore the utmost confusion in the minds of most persons as to the limits of these departments of thought. Some clearing up is sadly needed here. The nature of the clearing up which must eventually take place may, I think, be well illustrated by what has already taken place in other and simpler departments of thought. The history of chemistry in its relation to physiology is peculiarly instructive in this regard.

ILLUSTRATED BY THE HISTORY OF CHEMISTRY.

The older chemists made two widely distinct divisions of chemistry with supposed impassable gaps between, viz., mineral chemistry and organic chemistry. The one dealt with elements and with compounds synthetically derivable from elements or from other mineral compounds; the other dealt with substances which could not be made synthetically from mineral matter, whether elements or compounds, but only analytically from substances already made to hand by living organisms. Here, then, were two kinds of substances offered ready-made by nature, which the chemist might use as materials, but which he could not himself make. These were elements on the one hand and life-products on the other. Starting with the one—elements—he works synthetically upward to certain heights of complexity; starting with the other—life-products—he works downward analytically

to the same level or even lower, to the bottom; but it was supposed impossible to work up synthetically above a certain hard and fast line. Mineral substances therefore were such as are found in nature or derivatives from such ascensively or synthetically, or, in any case, into every step of the formation of which entered chemical forces only. Organic substances were such as are either life-products or derivatives from life-products, descensively or analytically—*i. e.*, they were substances in the production of which life entered as one link in the chain of preceding causes. Without life they could not be.

Here, then, seemed to be the boundary line between the mineral and the organic kingdoms. Here nature seemed to draw the line and say, "Thus far may your pure chemical action go, but no farther;" the rest can only be made directly or indirectly by life. Thus, then, the division between mineral and organic chemistry seemed sharp and easily understood. But, on the other hand, organic chemistry seemed to shade insensibly into physiology. Chemical changes are going on continually in the animal body. Are not these in the domain of organic chemistry too? Surely they are. The distinction was sharper between mineral and organic chemistry than between organic chemistry and physiology.

Such was the condition of things fifty years ago. But soon one of the substances of the so-called organic group was made synthetically from strictly mineral substances; then another; then still another. The line was readjusted each time, but still held. But soon triumph after triumph followed each other so quickly that readjustment was no longer possible. Finally the army of chemists broke over the line ,conquering and still to conquer, and in the first flush of victory claiming synthetic authority, not only over the whole territory of derivatives from life-products, but also over the original life-products themselves; and not only over these, but also over all nature, organic and inorganic, living and dead, thus abolishing entirely the whole science of physiology by absorbing it.

Let me present the argument as it seemed to these chemical philosophers. Every substance has its own definite composition, and indissolubly connected with this its own invariable properties. Now, some of the properties thus indissolubly connected with composition are very remarkable and even mysterious. For example: Iron united with oxygen in certain definite proportion s forms an oxide—magnetic oxide. Now, associated with this definite composition are the strange properties of attracting iron, of determining electric currents, and of pointing to the north pole. Is not this almost as wonderful as life itself? Now, protoplasm is a substance doubtless of definite composition, consisting of C. H. N. O., with a little S. and P., all in definite proportions, and connected with this composition is the wonderful property of motion responsive to impressions; in other words, of life. If, therefore, we could only make protoplasm we would thereby make a living thing. It is true we have yet conquered but a very small part even of the territory of the derivatives and are, therefore, very far from the conquest of original life-products, especially of that most complex and unstable product, protoplasm, in which alone life seems to inhere. But we have broken down the barrier, the rest is only a question of time and patient research. It is true the difficulty becomes greater and greater as we rise; but science is also becoming more and more powerful in her methods. To illustrate: The scale of chemical substances may be likened to a Jacob's ladder. Its base rests on the earthly plane of simple elements, its top reaches to the heavenly plane of living organisms. Chemists are the angels who ascend and descend this ladder by synthesis and analysis. It is true they are yet very far from reaching the top, but they are on the ladder. It is true the rounds grow farther and farther apart as we ascend, but our chemical legs are growing longer, too. Shall we not finally reach the highest round and get on the platform of life above? Shall we not eventually make life-products: starch, dextrine, cellulose, and even the highest, in which life itself inheres—protoplasm? If we make protoplasm

have we not generated the properties inherent in protoplasm, viz., life? Have we not, therefore, created life? And if we can create life, how much more may we hope to preserve life indefinitely? Then follows, of course, the revival, on a strictly scientific basis, of all the glorious dreams of the alchemists—Elixir of Life, Immortal Youth, &c. Why not?

Such were and still are, according to many, the claims of chemistry; and such were some of the glowing hopes naturally raised thereby—not, indeed, often expressed by scientific writers, for the scientific imagination is strictly restrained and chastened by reason, but mainly by the popular scientific folk. But more and more, as sober second thought gained control, a flaw was seen in this reasoning. Protoplasm is, indeed, the goal, perhaps the unattainable goal, but still the legitimate goal, of chemical synthesis. But is there not such a thing as dead protoplasm? When we speak of the composition of protoplasm as C. H. N. O. and S. P., is it not of dead protoplasm we are speaking? Can protoplasm be dealt with at all by the chemist? Does it at all fall into the domain of chemistry until it is dead? Whether there be any difference in composition between living and dead protoplasm we know not, and can never know, because chemistry cannot deal with it except in a dead state; but the difference in properties is inconceivably great. The difference, therefore, between living and dead matter is not a chemical difference, but a difference of another order. When, therefore, the chemist shall have reached, if ever, his utmost goal, the synthetic preparation of protoplasm, how shall he then quicken it into life? Not by chemical means.*

* It is possible, indeed probable, that the difference between living and dead protoplasm is a difference not of chemical equivalent composition, but of molecular arrangement, such as we find in allotropism. If so, then the allotropic condition which we call living protoplasm cannot be produced except in the presence of and in contact with previous matter of the same kind—*i. e.*, in contact with previous life. As a magnet by contact communicates its own property of magnetism to other iron, so living protoplasm by contact communicates its own property of life to other protoplasm under certain conditions.

Thus, then, as the line between mineral and organic chemistry was more and more effaced, the line between chemistry and physiology became clearer and clearer, chemistry claiming all dead matter and physiology all the living. The mutual claims of these two sciences may be said now to be permanently adjusted. There is, of course, a chemistry in the living body, but it is chemistry under the control of a higher force—life. It is true that both physics and chemistry underlie and condition life; but life-phenomena are a peculiar group; so peculiar and so different from pure chemical and physical phenomena that they constitute the subject-matter of a very distinct department of science.* It is worthy of remark that this is only a return, after a long scientific detour, to the original and most obvious point of view, for popular intuition has always recognized the great gap between the living and the dead. Thus in many ways science verifies our intuitions. This, in fact, is the most important function of science.

* It will be observed that all through this article I use the words *vitality*, *vital force*, *life-force*. Now, I know that this expression is often ridiculed as a remnant of superstition. It has even been wittily said that we might as well speak of *aquosity* as determining the properties of water as of *vitality* as determining the phenomena of living organisms. Such objectors are partly right and partly wrong. The old idea of vitality was that of a force standing above natural forces and controlling them, and thus in some sense supernatural and unrelated to other forms of force. This idea is untenable. Vital force is correlated with, derivable from, transmutable into, physical and chemical forces, and no one has insisted on this more than I have; but it is none the less a very distinct form of force, more distinct from them than they are from one another, giving rise to a very peculiar group of phenomena, which constitutes the subject-matter of a very distinct department of science, more distinct than any other lower department. As long as we use names for other forms of force, as gravity, electricity, chemical affinity, &c., much more ought we to use a name, *vitality*, to group the phenomena characteristic of living things. If it be asked, Why not then aquosity for the force inherent in water? I answer: This does not group phenomena belonging to an infinite variety of bodies. Affinity or chemism is the proper word here.

APPLICATION TO PHILOSOPHY.

I have dwelt on the adjustment of the boundaries of these two sciences, partly because it is in itself of great philosophical interest but especially because a similar change is now going on and must continue to go on in our ideas of the relations of physiology to the science which deals with the distinctive phenomena of the human spirit, and which I have called philosophy. We have, I believe, drawn sharply the line of lower limit of physiology—*i. e.*, its relation to chemistry; but where shall we draw the upper limit of physiology—*i. e.*, its relation to philosophy? The functions of plants and the so-called vegetal functions of animals—*i. e.*, the nutritive and reproductive functions of these—of course, belong exclusively to physiology. But the distinctive animal functions, those connected with the nervous system, what shall we say of these? Nerve-functions include brain-functions—even the highest—viz., cerebral functions; but indissolubly connected with cerebral functions are sensation, consciousness, volition. We cannot treat of nerve-functions without touching these also; and yet these are included in all our works on mental philosophy. See again the steps of nerve-action in animals: (1) Impression on a nerve terminal; (2) Transmission along a sensory fiber; (3) Change in a central sensory cell; (4) Transmission by a connecting fiber to a central motor cell; (5) Change in a central motor cell; (6) Transmission along a motor fiber; (7) Muscular contraction, producing motion. Now, all this may take place reflexly and therefore without the slightest consciousness or will. Evidently in that case the whole series belongs to physiology alone. But sometimes—*i. e.*, when the cerebrum is involved—there emerge from this series other phenomena, apparently of an entirely different order, viz., consciousness, sensation, will, emotions, desires, &c. Right here the line of separation used to be drawn and is still drawn by many, perhaps most, and philosophy becomes identified with psychology. All the changes in the reflex series given above

are conceivably and probably are really wholly molecular; but these latter are of a different order and cannot—at least as we know them in ourselves—by the utmost stretch of the imagination be construed in terms of molecular movement. Right here, therefore, there is supposed to be an unbridgeable chasm and therefore a prime division in science. Right here commences psychology, the science of the psyche, the soul, the spirit. All below this is molecular movement—kinesis—and belongs to physiology; all above is meta kinesis, and belongs to psychology or philosophy, for these are identified.

But as great as this chasm undoubtedly is, it is not so great as it at first seems, nor so great as another chasm to be mentioned presently. In fact, it is far less great than most imagine. There are many stages of gradation between the admittedly physiological unconscious reflex actions and the purely psychical conscious actions: (1) First, of course, there is the series of seven steps given above without the least consciousness or volition. These are called reflex. Such are the actions of the heart, stomach, intestines, &c. (2) Secondly, there are actions which are wholly removed from volition but not from consciousness; such are actions like swallowing, sneezing, &c. (3) Thirdly, are those which are partly but only partly under control of volition also, such as breathing. (4) Then follow instinctive actions, so wonderful in many animals. These are conscious and semi-voluntary, but not individually acquired. They are inherited habits. (5) Next come habitual actions, such as walking, talking, writing, playing on a musical instrument, &c. These are also semi-voluntary—*i. e.*, only the general result is willed—but they are individually acquired. (6) Next come the distinctly voluntary actions of the higher animals—*e. g.*, actions performed for the first time. (7) Lastly, and in man alone, come moral actions, where opposite motives are weighed and free, deliberate choice is made. Where, now, shall we draw the upper limit of physiology? Or shall not physiology claim the whole, including even the highest activities of the human

spirit, in reflective introspection, in abstract thought, in unselfish love, and in free moral choice?

This last extreme undoubtedly represents the tendency among modern materialistic thinkers; but here also, as in the other case, while the lower line of limit, viz., that between physiology and psychology, may, and I believe will, become more and more effaced, the upper limit line, viz., that between psychology, or the science of the animal soul, and philosophy, or the science of the human spirit, must grow ever stronger. As in the former case—*i. e.*, relation of physiology and chemistry—the greatest distinction is between the physically living and not living, so in this it must be between the spiritually living and not living, or perhaps we may call it the spiritually awake and dormant—*i. e.*, between man and animals. In the series given above by far the greatest chasm is between six and seven. As chemistry ranges up and down by synthesis and analysis, claiming the whole of dead matter to the very borders of the life plane, but there stops, though still underlying and conditioning life phenomena also, so physiology will eventually range up and down through all the phenomena of vegetal and animal life, claiming all psychology as her domain, to the very borders of the phenomena of self-conscious free spirit, but there must stop, though still underlying and conditioning these also. Observe, again, that this also is only a return, after a long scientific detour, to an original and obvious view, but placing it now on a secure basis.

PHILOSOPHY VS. PSYCHOLOGY.

If these views be correct, then must we make a wide distinction between philosophy and psychology—much wider than between psychology and physiology. It is true that at the present time psychology seems very distinct from physiology; but eventually it will, I think, be absorbed by that science and become only a higher department. Physiology is the science of life—vegetable and animal. It includes, therefore, all the phenomena of plants and animals. Psychology will eventually be identified with nerve-physi-

ology; but philosophy, which is the science of free, self-conscious spirit, belongs to man alone. There is a psychology in man, too, for man is also an animal; but all the activities of self-conscious, self-determining spirit, such as abstract thought, unselfish love, and free moral choice, and all that flow from these, belong to philosophy. There is, therefore, a comparative psychology as there is a comparative anatomy and a comparative physiology, and these comparative sciences include man also as subject-matter; but there is no comparative philosophy in the sense spoken of above. There is a comparison of philosophers, but not a comparative philosophy.

But I must not be understood as severing philosophy wholly from other departments of science. The distinction here made between free spirit and vital force even in its highest form, as nerve-force and psychic force, and between philosophy and psychology, although the greatest in nature, yet is similar to the distinctions between other forms of force and between other departments of thought. All forces are in a sense natural, and all natural forces are derived one from another in regular order by a process of evolution, though not necessarily by insensible gradations. Free spirit, I am convinced, is no exception to this universal law. Let me illustrate what I mean:

There was a time in the history of the earth when, as I suppose, only physical forces, gravity, heat, light, &c., existed. Perhaps first of all only gravity. Chemical affinity did not yet exist, being held in abeyance by intensity of heat. As the earth gradually cooled, at a certain temperature chemical force was born and chemical reaction suddenly began—a new kind of force, a new group of phenomena, a new possible science. Suddenly, I said, but not therefore underived from previous forces. On the contrary, physical forces under new conditions changed into, became chemical force. Ages passed away until conditions were favorable, and life appeared suddenly on the scene—again a new kind of force and a new and peculiar group of phenomena, furnishing materials for a new science, physiology. Suddenly again,

perhaps, but not therefore underived. On the contrary, I believe chemical force, under new and now unimaginable conditions, became vital force. As time went on and conditions became favorable, among these vital phenomena there suddenly appeared, how we know not, nor can even imagine, but somehow there appeared consciousness, will, feelings, desires—a new kind of force—psychic—a new and peculiar group of phenomena, furnishing material for a new science, psychology. Suddenly, I said, but not therefore underived. On the contrary, under certain conditions, vital force became psychic force. Time went on, ages upon ages passed, during which the psyche or animal soul took on higher and higher forms, simulating even reason itself, until finally, when the time was fully ripe and conditions favorable, suddenly—how we do not know, nor can we well imagine, but somehow—there appeared self-conscious, free spirit. Suddenly—from the very nature of the case it could not be otherwise than suddenly, but not therefore underived. On the contrary, the psyche became free spirit. By gradual development its birth-time came, and it was born into a new and higher world, the spiritual and immortal.

Now, that all these forces are really of derivitive origin is proved by the fact that we see every step of this process taking place now daily under our very eyes. I pass over the conversion of physical into chemical force because this is admitted on all hands. I begin therefore with vital force. In the green leaves of plants we see every day physical changed into chemical and chemical into vital force. Sunlight falling on green leaves disappears as light and reappears as life—is consumed in doing the work of decomposing CO_2 H_2O and $N H_3$—and the carbon, hydrogen, oxygen, and nitrogen thus set free from previous combination unite to form living protoplasm.* Again, in the embryonic history of every animal

* The origin of vital from chemical force in the green leaves of plants cannot be doubted, but this does not, of course, explain the first origin of life on the earth; for one condition of the change now is the contact of living matter.

we see the next change take place—*i. e.*, the emergence of the psychic out of the vital. In the germ-cell, in the egg, and even in the earlier stages of the embryo, there is no distinctive animal life—*i. e.*, no consciousness nor volition nor response of any kind to stimulus. At a certain stage, distinctive animal or psychic life appears. We call it quickening. Materials for psychology are now present for the first time. In man alone, and that only sometime after physical birth, we see the last change. The new-born child is possessed of animal life only. The emergence into self-consciousness—a change so wonderful that it may well be called the birth of spirit—takes place only at the age of two or three years. Now, for the first time, we have phenomena which are the subject-matter of philosophy.

Now all these forces, as we have shown, operate on separate planes without gradations. When appropriate conditions are present, force or energy changes from one form to another at once. The gaps separating these planes become greater and greater as we rise. The scale of forces, like the scale of chemical substances, may be likened to a ladder, the rounds of which grow farther and farther apart as we rise. By far the greatest gap between these successive planes or rounds is the last, or that between the psychic and the spiritual plane; but, although thus distinct, they are not unrelated. They are related to one another, as already seen, in their origin. They continue to be always related by mutual dependence. Each higher force as it rises dominates all below and uses them for its own higher purposes, but is itself underlain and conditioned by the lower. The sciences of which they form the subject-matter are therefore similarly related. As the physical underlies and conditions chemical phenomena, and therefore the accomplished chemist must understand physics; as the chemical underlies and conditions life-phenomena, and therefore the physiologist must understand chemistry; as vital forces underlie and condition psychical phenomena, and therefore the psychologist must understand physiology, so also psychical forces under-

lie and condition the phenomena of free spirit, and therefore must the philosopher understand psychology.

If these views be correct, then Des Cartes and Huxley are right in regarding animals as in some sense conscious automata, and very much of human action also undoubtedly comes in the same category. But there is this difference between these two thinkers: Des Cartes excludes man in his highest activities from this automatism. In this I think he is right. Huxley, on the contrary, extends automatism to include all the activities of man. In this I think he is wrong. The phenomena of self-conscious spirit alone are free and self-determined.

Finally, we have said that philosophy alone is the science of free spirit. Is there, then, it may be asked, no activity of free spirit in other departments of thought besides philosophy? Surely there is. All science and fine art, all ethics and religion, as well as all philosophy, are the products of the activity of free spirit, and are therefore strictly confined to man. But there is this difference: Science, as we usually define it, and art are the result of the activity of free spirit on materials furnished in the first instance from without by the external world. Philosophy is the result of the activity of the same free spirit on materials furnished by itself from within—from the inner world of self-consciousness. Ethics and religion deal with materials of both kinds. The difference is only in the subject-matter, not in the kind of activity.

Will some one say that the view above presented is a return to the old, high, metaphysical view of philosophy, and therefore a step backward from the new scientific view? Well, perhaps it is so to some extent, but it is also a limiting it to its proper domain and placing it on a rational basis. At the same time it gives full play to the new psychology and defines also its domain. It differentiates these two from one another and places each on its true line of progress. Evolution and progress are not made, as many suppose, by exploding of old views and substitution of new, created at

once out of different materials, but by modification of the old and its incorporation into the new. The best test of a rational philosophy is that it continues and reconciles opposite extremes by explaining their differences. The view above presented, I believe, stands this test.

THE MEXICAN METEORITES.

BY

JOHN ROBIE EASTMAN.

[Read before the Society, January 2, 1892.]

For many years remarkable accounts of wonderful specimens of iron meteorites, discovered in Mexico, have been scattered through nearly all the principal chemical journals and in many of the astronomical publications of the world. Great confusion is constantly arising in the literature of meteorites from the fact that a large amount of the data is derived from persons who have no definite ideas of time, direction, distance, locality, or weight; in fact, so widely divergent are the various descriptions of the same meteoric mass that it seems almost certain that the several accounts refer to widely separated and totally unlike bodies.

In no locality has this confusion of information reached a more exasperating stage than in Mexico. The mineralogist, the man who travels for pleasure, the miner, the adventurer, and the pseudo-scientific man, who knows a little of many things and nothing well, have all brought to the general stock of information their contributions of special, partial, and distorted data mixed with hearsay evidence of localities " over the mountain " until one is led to doubt all statements till they are confirmed by the sight of the meteorite and by the chemist's analysis.

Very gratifying progress, however, has lately been made in unraveling the tangled and often contradictory accounts of the Mexican meteorites by the publication of two important papers on this subject.

In 1889, Antonio del Castillo, director of the School of Mines of Mexico, published a descriptive catalogue of meteorites.*

In addition to the information announced in the title of this catalogue, the author gives the weight and the present location of many of the masses found in Mexico.

This catalogue was followed in 1890 by a very important paper † by L. Fletcher, F. R. S., keeper of minerals in the British Museum, "On the Mexican Meteorites, with especial regard to the supposed occurrence of wide-spread meteoric showers." This paper was accompanied by two maps of the principal localities mentioned. The title fully sets forth the character of the paper, but it necessarily fails to give an adequate idea of the immense amount of research involved or of the careful manner in which it was carried out. It is well known that the opinion was held by Professor J. Lawrence Smith, and accepted by many other writers on the subject, that masses belonging to a single meteor were scattered over many hundreds of square miles of territory in northern Mexico. It was found that immense fragments of meteoric iron, separated by hundreds of miles, presented almost exactly similar chemical characteristics, and this was accepted as strong evidence of a common origin in a single meteorite which, by successive explosions while in motion, distributed its fragments over areas equal to those of the largest Mexican states.

Mr. Fletcher has spared no pains in examining all the available evidence, and concludes that there is no good reason for supposing that any very wide-spread dispersion has resulted from a single meteorite. He thinks it probable, however, that a large shower of masses with limited dispersion did occur in the valley of Toluca, a similar one in the state of Coahuila, and one in the state of Chihuahua. In the

* "Catalogue Descriptif des Météorites (Fers et Pierres Météoriques) du Mexique, avec l'indication des localités dans lesquelles ces météorites sont tombés ou ont été decouverts." Par Antonio del Castillo, pp. 15. Paris. 1889.

† Williams & Strahan, 7 Lawrence Lane, Cheapside, London.

latter location the maximum dispersion, according to present indications, was not over sixty-six miles.

The principal object of the paper which I present this evening is to bring together in as compact form as possible the latest and most complete information in regard to the Mexican meteorites and to place the data in convenient form for reference.

Much of the data here presented will correct and supplement some of the tabular matter in regard to Mexican meteorites that occurs in a former paper by the present writer and read before the Philosophical Society April 12, 1890.*

The results of the investigations in the two papers already referred to have been freely used, as well as information from other sources, while no small amount of time has been bestowed upon my own investigations. The paper by Fletcher contains copious references to the bibliography of the subject, rendering such references in this paper quite unnecessary.

The actually observed falls of meteorites in Mexico have been very few. The following list is believed to contain all that have been well observed:

1. November 24, $\left\{\begin{matrix}1704,\dagger\\1804,\ddagger\end{matrix}\right.$ at Hacienda de Bocas, state of San Luis Potosi.	
	k.
2. January, 1844, at Loma de la Cosina, state of Guanajuato	1.2
3. June, 1865, at Hacienda d'Avilez, state of Durango.	
4. June 11, 1878, at Irapuato, state of Guanajuato	0.40
5. September 17, 1879, state of Jalisco.	
6. June 18, 1881, at Pacula, state of Hidalgo	3.36
7. November 27, 1885, at Mazapil, state of Zacatecas	3.95

Number 7 of the above list is an iron meteorite and fell during the meteoric shower of November 27, 1885. It is the only recorded fall of an iron meteorite during a periodic shower, but it is not likely that it had any connection whatever with the small bodies producing the shower of November 27. The first six numbers on the list are stony meteorites.

*The Progress of Meteoric Astronomy in America, Bull. Phil. Soc. of Washington, 8°, Washington, 1889–1892, vol. xi, pp. 275–358.

† Castillo, 13. ‡ Fletcher, 5.

The following list contains a brief account of the discovered iron meteorites of Mexico condensed from all the sources of information to which I have had access. The estimated weights as originally given, except where accompanied by careful measurements, are probably very much exaggerated, but where any valid clue has been given to the size of the mass the weight has been carefully computed. In most instances those masses which are most widely known are recognized by the name of the discoverer or the person who brought it within the range of scientific examination, or by the name of the locality where it was found, and it has been deemed advisable to retain these names as far as possible.

DISCOVERED METEORITES.

State of Coahuila.

1. *The Bonanza Masses.*—These masses, *fourteen* in number according to one authority, and *thirteen* according to another, were said to have been located in an open space about one-quarter of a mile square. The top of the largest was said to be four feet above the surface of the ground; it was shaped like a bee-hive, and was five feet in diameter where it entered the ground. It is remarkable that nothing more definite has ever been ascertained about this enormous mass of iron. If the above description is true the mass must have weighed at least 13,600 kilograms.

2. *The Butcher Masses.*—These masses were brought from Mexico by Dr. H. B. Butcher, of Philadelphia. They were described by Dr. Butcher before they left Coahuila as consisting of "*eight* pieces, varying from 290 pounds, which is the smallest, to 654 pounds, which is the largest, making a total of nearly 4,000 pounds." So far as I can learn, only six pieces reached the United States at first, and they weighed respectively 290, 430, 438, 550, 580, and 654 pounds, or, in the aggregate, 2,942 pounds. Most of these masses came into the hands of Professor J. Lawrence Smith and finally went to the Museum of Harvard College. Subsequently, two other

masses of the same group, weighing 353 and 450 pounds, were brought from Mexico, but it is not known what became of them.

3. *The Santa Rosa Mass.*—This mass, now in the mineralogical collection of the National School of Engineers in Mexico, weighs 63 kilograms and was found at Muzquiz. It was probably brought to Muzquiz from some locality now unknown.

4. *The "Couch" Meteorite.*—This well-known mass, now in the National Museum at Washington, was said to have been found at the Sanchez estate, about 50 or 60 miles from Santa Rosa. It weighs 114.3 kilograms.

5. *The Fort Duncan Mass.*—This has frequently been classed with the Mexican meteorites, but it was discovered by Lieutenant Cusick, U. S. A., on the Texas side of the Rio Grande river, more than 110 miles from the nearest point in Mexico where meteorites have been found. It weighs 44.1 kilograms.

State of Nuevo Leon.

6. *The Potosi Mass.*—This mass was seen by Weidner at the hacienda of Potosi, district of Galeana. Weidner mentioned this mass in 1870 and estimated its weight at 91 kilograms.

7. *The Cerralvo Mass.*—This was seen by Peter McDermot in 1847 in use as an anvil at Cerralvo, in the district of Oriente. He estimated it to be very much like the Orange River meteorite, which weighed about 136 kilograms.

State of Chihuahua.

8. *The Casas Grandes Mass.*—This was found in the ruins of Casas Grandes, wrapped in coarse cloths, and has been described as "a lenticular mass 50 centimeters in diameter." Another account says "it measures 2 feet 6 inches square and is supposed to weigh 5,000 pounds." The present location of this meteorite is not known.

9. A mass of meteoric iron was shown among the Mexican minerals at the United States Centennial Exhibition, in Philadelphia, in 1876, and afterwards brought to its present resting place in the National Museum, in Washington.

The maximum dimensions of the mass are 37.5 × 24.5 × 17 inches. So far as known, it has never been weighed, but it will probably weigh about 1,134 kilograms. The locality whence it came is entirely unknown, but I think it highly probable that it is the Casas Grandes mass.

10. Cornejo, in 1869–70, and Barcena, in 1876, mention the occurrence of meteoric irons in the vicinity of Presidio del Principe, in the state of Chihuahua, but nothing definite is known about them.

11. The Huejuquilla group of meteorites was originally known as the "Durango iron," on account of Humboldt's erroneous supposition that one of the masses which he described was found near the city of Durango. Originally there were reckoned seven of these masses:

(1.) Now at the hacienda of San Gregorio; estimated weight, 11,560 kilograms;

(2.) Now at the hacienda of Concepcion; estimated weight, 3,130 kilograms;

(3 and 4.) Now at the hacienda of Chupaderos; estimated weight, 15,600 and 9,290 kilograms;

(5.) A small fragment brought from Sierra Blanca;

(6.) A small fragment brought from the rancho of Tule;

(7.) A large mass said to have been seen many years ago at Rio Florido, but nothing known of it now. Although Weidner reports to Burkart in 1870 that "during a journey to Chihuahua I saw two masses, one at the hacienda la Florida, the other at the hacienda Concepcion. Without delaying the caravan, I was able to hammer off a small piece of the *former* meteorite," etc., etc., and in 1866 reported to Castillo that the Florido mass was about half the size of the Concepcion mass, it is quite probable from Fletcher's investigations that the two descriptions refer to the same mass.

State of Sinaloa.

12. The Ranchito mass was discovered in 1871, about 3 miles southeast of Bacubirito, which is 127 miles southwest of El Valle. The mass is said to measure $3.65 \times 2.0 \times 1.5$ metres. If these measures are correct, it probably would weigh about 40,800 kilograms.

State of Durango.

13. The La Plata mass was discovered 500 yards northeast of a hill called La Plata, in the Sierra Madre, 13 or 14 leagues from the city of Durango. Its original weight was about 124.7 kilograms, and it was first mentioned in the "Gazetas de Mexico" for 1802. A part of the mass was made into a plowshare and the remainder has disappeared.

14. *The Guadalupe Masses.*—(1) One mass mentioned by Karawinsky, and supposed to have been cut from a mass weighing several hundred pounds, was found lying in the plain northeast of Durango. This fragment is now in the Vienna collection. Its history is very doubtful.

(2) A second mass was plowed up in 1882 at the rancho of La Pila, nine leagues east of Durango. It is now in the British Museum collection and weighs 46.4 kilograms.

15. The Cacaria mass, weighing 41.4 kilograms, was in use as an anvil for several years at a blacksmith's forge in Durango. It was carried to the city of Mexico before 1870 and is now in the Mexican Museum.

16. The Mezquital mass was taken from Mexico before 1868. Its discoverer and its history are not known, but it is said to have come from San Francisco del Mezquital. Its original weight was about 7 kilograms, of which the larger portion is now in the British Museum collection.

17. The Bella Roca mass was found on La Bella Roca, a peak of the Sierra de San Francisco, about 85 miles northwest of Durango City; date of discovery unknown; was obtained by Professor Henry A. Ward, of Rochester, N. Y., in 1888; weight, 33 kilograms.

State of San Luis Potosi.

18. *The Catorce Masses.*—(1.) The Descubridora mass, weighing 576 kilograms, is said to have been discovered between 1780 and 1783. It is now in the National Museum in the city of Mexico. The " Venagas " mass described by J. Lawrence Smith is supposed to be identical with the above.

(2.) Between 1828 and 1834 Burkart saw at Zacatecas, in the possession of an Italian named Chialiva, a complete mass, rounded in form, weighing " 10 or 12 pounds," and said to have come from Catorce. Nothing is known of it since.

(3.) A mass weighing 41.7 kilograms was found by a miner near Catorce in 1885. It is now in the collection of G. F. Kunz.

19. *The Charcas Mass.*—Sonnenschmid, in 1804, says this mass was situated at the corner of a church-yard at Charcas and had been brought there from the neighborhood of San José del Sitio, about 12 leagues distant. This location cannot now be identified. This mass, which weighs 780 kilograms, was carried to France in 1866 and is now in the Paris collection of meteorites.

It is not improbable that the Catorce and Charcas masses came originally from the same locality.

The State of Zacatecas.

20. The Zacatecas mass in 1792 was in St. Domingo street, in the city of Zacatecas, and was said to have been found by one of the early colonists when working the Quebradilla mine on the western outskirts of the city. It weighs 907 kilograms.

The State of Mexico.

21. *The Toluca Group of Meteoric Irons.*—Numerous masses of meteoric iron, varying in weight from 300 pounds to minute specimens, have been found in the Toluca valley, and it is highly probable that they all came from the vicinity of Xiquipilco, in the state of Mexico. Many of these masses were used by the native blacksmiths for making agricultural

implements. Many specimens are in the various European collections and at least fragments are in the collection in the city of Mexico. In 1856 Gustav A. Stein reported that in 1854 he visited the country in the vicinity of Xiquipilco to learn more of the meteoric irons. He there got information of at least four specimens, weighing in the aggregate 545 pounds. The weight of all the specimens found in that vicinity can never be known. It is probable that the specimen in the Poinsett collection was from Toluca valley.

The State of Morelos.

22. A specimen of iron from Los Amates forms a part of the collection of Castillo, and a specimen from Cuernavaca is in the National Museum of Mexico. Castillo gives no weight for either specimen.

The State of Oaxaca.

23. The Yanhuitlan mass is said to have been found by Indian laborers while tilling the ground at the foot of a hill near Yanhuitlan. It was used by a blacksmith for several years as an anvil. In 1864 it was removed to the city of Mexico and is now in the National Museum of that city. Many small fragments were removed before it was carried to Mexico, but its weight is now 421 kilograms. The so-called Cholula mass, the Chalco mass, and the Misteca Alta fragments all probably refer to the Yanhuitlan mass.

The State of Guerrero.

24. The Caparrosa mass was found in 1858 by Castillo on breaking a piece of copper pyrites which had been brought from the Rincon de Caparrosa. It was a nodule of metallic iron weighing 341 grams, and when etched it showed Widmänstatten figures.

The information in regard to many of these meteorites is far from being as complete as it should be.

There is still great uncertainty about the Bonanza masses and the Rio Florido mass, while nothing very definite is known about the irons said to be in the vicinity of Presidio del Principe. The actual size and weight of the Ranchito mass in Sinaloa is still very uncertain.

The discovery of the truth about these bodies will give ample scope to the energy of any enthusiastic and competent explorer and investigator, and the sooner the work is undertaken the better will be the chances of success.

Tabulated Weights of the Mexican Iron Meteorites.

	Kilograms.
1. The Bonanza masses (estimated at least 15 tons)	13,600
2. The Butcher masses	1,699
3. The Santa Rosa mass	63
4. The "Couch" meteorite	114.3
5. The Fort Duncan mass	44.1
6. The Potosi mass (estimated)	91
7. The Cerralvo mass (estimated)	136
8. The Casas Grandes mass (estimated at 5,000 pounds).	
9. The Centennial Exhibition mass, probably same as No. 8 (estimated)	1,134
10. The Presidio del Principe masses, no weight known.	
11. The Huejuquillo masses, San Gregorio	11,560
" " Concepcion	3,130
" " Chupaderos	15,600
" " "	9,290
12. The Ranchito mass, measures 3.65 × 2.0 × 1.5 meters.	
" " (estimated)	40,800
13. The La Plata mass	124.7
14. The Guadalupe masses	46.4
15. The Cacaria mass	41.4
16. The Mezquital mass	7
17. The Bella Roca mass	33
18. The Catorce masses, 576, 4.5, 41.7 kilograms	622.2
19. The Charcas mass	780
20. The Zacatecas mass	907
21. The Toluca mass (estimated)	275
22. Specimens from Los Amates and Cuernavaca (no weight given).	
23. The Yanhuitlan mass	421
24. The Caparrosa mass, 341 grams	0.3
Total	100,519.4

The relative occurrence of iron and stony meteorites is of such a notable character that it deserves some consideration, especially in connection with the meteorites of Mexico. The iron meteorites contain no constituents which are not found under other conditions in the earth's crust; but, while the peculiar combinations of iron, nickel, and other elements placed these bodies in a special class, they did not furnish sufficient evidence in themselves of an extra-terrestrial origin.

This evidence was discovered, however, in a very conclusive manner when iron meteorites were seen to fall in the same manner as stony ones had been observed. These iron bodies, whose fall had been witnessed by many trustworthy people, were found to contain the same constituents combined in the same proportions as those similar bodies that had been discovered on the earth's surface. The iron and nickel masses occupy one extremity of the meteoric scale and the so-called stony bodies the other. From the astronomical standpoint it is exceedingly difficult to draw the line that separates the two classes. It is almost, if not quite, impossible to find a stony meteorite of any considerable size that contains no trace of iron. From specimens containing grains of iron only visible with a microscope there is an uninterrupted gradation to those containing nodules ranging in diameter from a millimeter to a centimeter. When a stony meteorite falls to the earth it generally breaks into many fragments either just before or at the time of its collision with the earth's surface, and in most cases the ruptured surfaces plainly indicate the nature of the catastrophe.

I know of no case where an iron meteorite shows any indication of having been broken, twisted, or torn off from another mass of the same material. If the mass is known to have traversed a long path in the earth's atmosphere it will be found with one surface, presumably that of the preceding end or side, exhibiting a modified form, in some instances showing the effect of heat of sufficient intensity to partially fuse the outside of the mass, but there is no evidence of rupture by tearing away from another body. For many years

the village blacksmiths in some parts of Mexico have used fragments cut from meteoric masses for making horseshoes and agricultural implements as they would use the ordinary bar-iron of commerce, giving it the preference above other iron because of its extreme toughness and durability. The recent experiments in the use of nickel-steel plates to resist the action of modern high-power guns would seem to confirm the experience of the country blacksmith.

From the nature of the material and from the absence of any definite proof, it seems unlikely that these various masses of meteoric iron have been produced by tearing any large mass of iron into fragments.

On the other hand, it is highly probable that a meteoric mass like that which fell in Iowa county, Iowa, on February 12, 1875, is the true type of the meteorite which reaches the earth from outer space. This meteorite is composed almost wholly of stony matter, but scattered all through the mass are small grains of iron containing sufficient nickel so that the iron surfaces have not tarnished since they were polished soon after the body struck the earth. There seems to be no valid objection to the theory that these specimens of nickeliferous iron may exist in the stony matrix in all forms and sizes, from the microscopic nodule to the mass weighing several tons. Whenever the stony mass with a high velocity comes in contact with the earth's atmosphere the sudden and intense heat developed, together with the shock of impact, breaks up the less tenacious matrix, sets free the iron bodies, and they reach the earth in nearly the same condition, so far as mass and figure are concerned, as they existed in the original formation. In such cases it is probable that the stony portion of the original body is rent into such minute fragments by the explosion, due to unequal heating and to collision with the atmosphere, that they would not reach the earth in any appreciable size. The larger the masses of iron the more complete would be the destruction of the original body, and the larger stony meteorites would be those that contain the smaller granules of iron.

It is comparatively easy to detect a mass of meteoric iron on the surface of the earth on account of its weight and appearance, but it is extremely difficult for any one to recognize a stony meteorite except after careful examination. For these reasons nearly all the discovered meteorites are composed principally of nickeliferous iron.

It is of some interest, therefore, to ascertain from the available data the probable quantitative relations of the two classes of meteorites.

Masses of meteoric iron have fallen on this continent on four different occasions. The aggregate weight of these irons is 96.7 kilograms. Other falls have been reported, but those phenomena are not well attested. The total weight of stony meteorites whose fall has been observed on this continent is 1,192.3 kilograms. From this data it appears that the ratio of the weight of iron to stony meteorites is as 1 to 12.33.

The aggregate weight of meteoric iron observed and discovered to date on this continent is 138,590 kilograms or about 153 tons.

If the above ratio of iron to stony meteorites obtains in all these cases there should have been, to correspond to this mass of iron, a fall of about 1,880 tons of stony meteorites, or in all, of both classes, over 2,000 tons of meteoric matter precipitated upon the earth.

SOME PECULIARITIES IN THE RAINFALL OF TEXAS.

BY

ADOLPHUS WASHINGTON GREELY.

[Read before the Society February 13, 1892.]

The popular fallacy obtains that Texas has only one climate, whereas, in truth, there is no political division of the Union, except California, which presents to the investigator or the settler a greater diversity of climatic characteristics with reference to temperature, rainfall, wind, cloudiness, and humidity. It is not unusual to hear Texas spoken of as an empire in itself—a vague, indefinite term, which is applied to the detriment of Texas, whether as regards its physical area or its future possibilities with reference to its population or resources. As calculated by the United States Geological Survey, the total area of Texas is 265,780 square miles, or nearly one-eleventh of the area of the United States, Alaska excluded. It is larger than Great Britain, Ireland, and Italy combined, and considerably exceeds in area France, Germany, or Austria-Hungary. It is divided into two hundred and twenty-eight counties, of which no less than fourteen are larger than the State of Delaware. The geographical limits of Texas extend from 25° 50′ to 36° 30′ north latitude, while the extreme eastern and western boundaries are in respectively 93° 30′ and 106° 40′ west longitude. Geographical terms in this instance, however, convey very indefinite impressions to the general reader, or indeed to the ordinary student, since the extensive area of Texas forbids its representation in ordinary geographies by maps of the

same scale as other States, except as charted in continental areas or with countries of the world. It will assist, perhaps, in giving some faint relative idea of the extent of the State to remark that its extreme western portion, in El Paso county, is about 200 miles nearer the Gulf of California than the Gulf of Mexico, and that a portion of the Panhandle—a local name given to the portion of the State contiguous to and north of the 35th parallel of latitude—is nearer to the Great Salt Lake of Utah than to the nearest sea water, the Gulf of Mexico. It is as far from El Paso to the eastern boundary of the State as from New York to Chicago, and as far from the extreme northwest corner of the Panhandle to the southern border at Brownsville as from Chicago to Mobile, Alabama. While Texas has a longer stretch of sea-coast than any other State in the Union (California and Florida excepted), there are large areas of the State farther distant from the ocean than many inland political divisions, such as Idaho, Nevada, Kentucky, Tennessee, and Ohio.

The popular idea marks Texas as a country of plains and low elevation, yet about an eighth of the State is more than 3,000 feet above the level of the sea, while considerable areas exceed 6,000 feet. In general, nevertheless, Texas may be said to be a prairie country, rising very gently, with occasional interruptions of low ranges of hills, from the ocean surface to the 3,000-foot contour line of elevation. West of the 101st meridian, however, the Llano Estacado or the Staked Plains rise quite sharply from the 3,000-foot line to a nearly level plateau having an elevation of about 4,500 feet, and in the Trans-Pecos region scattered mountain ranges rise very abruptly from an elevation of 4,000 feet to an elevation of more than 6,000 feet. So gentle is the rise that, in general, the rate does not exceed five or six feet to the mile, while even in exceptional cases the average rate from the sea does not exceed ten feet per mile.

With its eleven degrees of latitude and marked contrasts of elevation, subjected to the widely dissimilar meteorological conditions arising on the one hand from a semi-tropical

ocean, inducing on the coast almost insular climate, and on the other hand from elevated and woodless plateaus favorable to the violent diurnal and monthly variations characteristic of a continental climate, such diversities of environment exercise in turn, through seasonal or accidental action, a dominating influence over the climatic elements of the whole or a great part of Texas.

It thus occurs that the State includes within its limits extensive areas where snow and ice are practically unknown, and others where prolonged and exceptionally severe "Northers" are accompanied by very low temperatures—far below zero.

As regards rainfall, considerable sections of the State have less than an inch a month, and so belong to the arid regions, while other localities have more than an inch a week. The rainfall is likewise subject to such extreme variability that the amount in one section during a single month may be triple the average amount in other sections for the entire year.

The importance of the phenomena of rain to a great agricultural domain such as Texas is scarcely second to that of temperature. It has been unfortunate for Texas in the past that rainfall observations have been few and far between. Indeed, apart from the records made by the Medical Department of the United States Army, at the various military posts, and those of the Signal Corps at its military telegraph and meteorological stations, other observations of rainfall are so widely separated and for such broken or brief periods that they by themselves would not give even a general idea of the amount of rain yearly or of its distribution over the State. The paucity of records other than those of the United States Army may be best summed up in the statement that there are only nine sets of continuous records by private parties covering five years, and not one of the nine points is situated outside of the eastern third of the State.

Notwithstanding that rainfall is comparatively local, yet from quite an open network of stations, especially over a

practically prairie country such as is most of Texas, can be formed a fairly accurate judgment of the precipitation of the intervening regions.

The average annual rainfall of Texas varies from about 52 inches in the extreme eastern counties to about 10 inches in El Paso county. The largest annual rainfall, as determined from records of considerable length, are Clarksville, 12 years, 57.13 inches; Galveston, 21 years, 52.33 inches; the smallest, El Paso, 22 years of miscellaneous records, 9.37, and Fort Stockton, 16 years, 15.94 inches. In connection with the El Paso rainfall it should be noted that for twelve years ending with 1890 the annual mean rainfall, as determined from Signal Corps records, is 10.54 inches. This latter mean is doubtless the more reliable.

It needs but casual examination of the available data to decide on or to verify the opinion that the average yearly precipitation decreases from east or southeast to west and northwest. In other words, the rainfall diminishes in quite regular proportion as the distances increase from the primary source of rain, which for the greater part of Texas is the Gulf of Mexico. Such decrease in the yearly precipitation also coincides, as a general rule, with increasing elevation—an incident and not a cause.

It has been suggested that the rainfall over the Llano Estacado is more copious than over the country lying immediately to the eastward, but all accessible data indicate the incorrectness of this theory or supposition, except where the Trans-Pecos type of rainfall overlaps that of central Texas. Doubtless in this case, as in other sections of the United States, the amount of local rainfall is somewhat affected by the configuration of the adjacent country. However, save in the mountainous regions of extreme western Texas, it is not probable that the rise in elevation is sufficiently rapid to compensate by its diminished temperature for the advantages arising from the increased distance from the Gulf of Mexico or the Gulf of California, according to the regularly varying or to accidental meteorological conditions. In any event,

no reasonable doubt exists that in very great measure, if not in its entirety, the rainfall of extreme western counties of Texas is derived from the supplies of aqueous vapor drawn northeastward in summer from the Gulf of California by the wind circulation arising from the low area of atmospheric pressure in the valley of the lower Colorado.

In some States of the Union the distribution of rainfall through the year is so equable that there is no season of water scarcity, while in others abundant precipitation begins at the commencement of or continues during the agricultural season.

In Texas, however, the conditions are unusual, as in the northwestern half of the State water is not so plentiful, while along the Gulf coast and in the extreme western counties a disproportionately large amount falls after agricultural products, or the greater part thereof, have attained their maturity or been harvested.

In consequence the distribution of precipitation through the year assumes in Texas an unusual importance, and merits careful consideration. Even in sections where irrigation is employed for agricultural purposes, it involves a very material difference to the investor and to the farmer whether the water to be conserved and distributed falls just prior to the season during which it is needed, or whether the water must be gathered after the growing season and be protected for months against rapid evaporation and seepage until the planting time of the coming year.

In view of this importance, an attempt is now made to resolve the general features of the annual rainfall distribution of Texas into their constituent elements, and to briefly define the characteristics and limits of three separate types, each of which singly dominates large areas of the State. In passing, it is to be remarked that from personal research and investigation, I am prepared to maintain that there is no published chart showing the summer rainfall of western Texas with any degree of accuracy, even with regard to general principles. So far all meteorologists have failed, for

the good reason that they have apparently not recognized the source whence comes the rain.

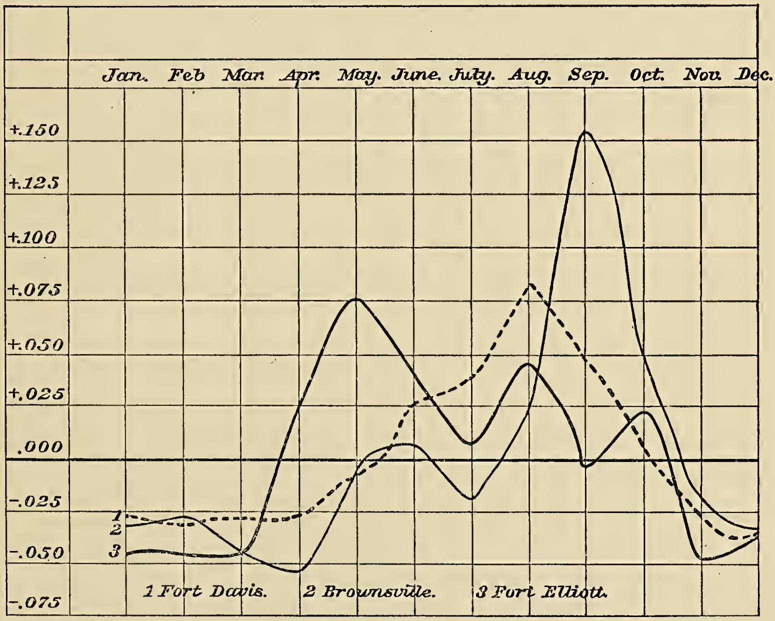
Even over a very extensive range of latitude the rainfall régime is sometimes, as in the case of California, so thoroughly similar that a composite curve can be drawn which will outline with general accuracy both the definite distribution throughout the year and the proportionate amounts pertaining to individual months. In Texas, however, a single composite curve, formed from observations representative of all sections of the State, would convey misleading and incorrect ideas for the greater part of the whole area.

Only a cursory consideration of the rainfall data is essential to be assured, as one would surmise from theoretical considerations concerning so vast a region as Texas, that no single type of distribution throughout the year applies to the whole State.

Instead of one or two composite curves, it was necessary in this case to reproduce quite a number of a representative character, based on the average daily rainfall (in hundredths of an inch) at suitable separate points in or adjacent to Texas. These curves, derived from a long series of observations by observers of the Signal Corps at each station, graphically represent the mean rainfall distribution characteristic of the station in question and of the country adjacent thereto. Only three of these curves are here presented.

If the annual rain fell regularly throughout the year it is evident that 8.3 per centum would fall each month. I have elsewhere designated as a *dry* month one in which the precipitation is not equal to 4.2 per centum, and a *very dry* month one in which the rain was not equal to 2.1 per centum of the annual precipitation. In like manner, a *wet* month has from 13 to 16 per centum, and a *very wet* month more than 17 per centum of the rain for the entire year. Under this scale July and August are, relatively speaking, *very wet* months for the Trans-Pecos region, while December and January are *very dry* months through all western Texas. Along the Gulf coast September is also a *wet* month.

In the coast region of Texas, say such part as lies within 200 miles of the Gulf of Mexico, the rainfall régime is that which has been elsewhere classified by me as the Gulf type. In this type the average rainfall during the months of August, September, and October is far above the amount which would occur if the distribution was equable through the entire year. In September, the maximum month, about one-sixth of the rain of the year may be expected, while in the minimum month, April, not more than a twentieth, or even much less, of the annual precipitation usually falls. At Key West, Florida, which affords the best example of this type, not less than 15 per centum of the annual precipitation occurs in September, while in March only 2 per centum may be expected. The increase in rain is steady from March to September, whence an unbroken decrease obtains to March.



The Gulf type is best represented by Brownsville, although the rainfall curves of Galveston and Indianola substantially accord therewith. On the diagram herewith, in thousandths

of an inch in rainfall, the plus and minus departures from the average daily rainfall for the year are charted for the separate months. In September the excess for each day amounts to .157 inch, while in April the deficiency is equivalent to .055 inch. As may be observed, the salient features are the very great excesses during September and October, and the decided deficiencies from December until their culmination in April. It is further to be observed that plus values obtain only during September, October, August, and June, in the order named, the latter doubtless resulting from the intrusion of the Trans-Mississippi type, which will be alluded to later.

At first sight one might naturally attribute the excessive mean rainfall of September to the advent of West India hurricanes; but while the potent effect of these violent storms as regards the autumnal precipitation is evident in certain years, yet a closer examination of the data shows that such abnormal distribution of the rainfall is not local but is quite general along the entire coast of the Gulf of Mexico.

Moreover, an examination of the storm paths from 1874 to 1890 indicates that over three-fourths of the low-area storms bringing precipitation to Texas originate on land. This rain-regime, then, of the coast region of Texas is not strictly local, but pertains to the whole Gulf of Mexico, and does not depend on hurricanes.

In the Panhandle, and thence southward over the central meridian of Texas, the month of greatest precipitation falls in the early summer (May), and the least in midwinter (January), thus conforming to the Trans-Mississippi type, represented best by Omaha, where over one-sixth of the annual precipitation falls in June and only about one-fiftieth in January.

In Texas the Fort Elliott curve represents best the Trans-Mississippi type with its very prominent excess of .077 inch during May, and the decided and persistent deficiencies from November to March, inclusive, the values of the daily rainfall during these months not differing in the extreme more

than .006 inch, practically nil. Your attention is invited to the abrupt increase of August, due to the intrusion of the Trans-Pecos type, and a less decided increase during October, probably resulting from the interference of the Gulf type.

In lower New Mexico and the Trans-Pecos region of Texas another type of rainfall obtains, which at first glance would appear to be the Gulf type slightly modified by the increased distance inland. The month of maximum precipitation is in late summer, August or September, and the rainfall is proportionally even greater than that of the coast of the Gulf of Mexico. The minima occur somewhat irregularly, according to locality, from December to May. It is, however, a different type that will be referred to as the Trans-Pecos. While it obtains from Fort Stockton, near the Pecos, westward to El Paso, its best example is the Fort Davis curve, herewith presented. The most marked characteristic is the extremely disproportionate excess during August (.087 inch), which exceeds by more than 75 per cent. the next greater excess (.049 inch) in September. The winter deficiencies are more persistent than those of the Panhandle, as represented by Fort Elliott, extending from November to April, inclusive, with an extreme difference in the values of these months of only .008, an inappreciable amount.

It appeared remarkable on first consideration of the subject that extreme western Texas should be favored with a far greater part of its rain in summer, and in connection therewith should have during July and August a larger number of rainy days than is experienced in the eastern part of the State. A thorough investigation proves quite conclusively that this western region shares the rainfall régime of New Mexico and western Arizona, and a further examination makes it evident that the ultimate source of precipitation in the Trans-Pecos type of rainfall is not the same as that for eastern Texas—the Gulf of Mexico.

No doubt exists in the mind of the writer that the summer rains of Arizona and southern New Mexico depend on abnormal barometric conditions, central during that season in

the valley of the lower Colorado river, which are represented best by the records of Yuma. This barometric depression naturally induces on its eastern side the southerly vapor-bearing winds characteristic of these territories from June to September. The resulting rain falls partly by the cold of elevation and partly through feeble offshoots of low pressure which drift eastward from the permanent depression.

As is well known, the barometric gradient between San Diego, California, and Yuma, Arizona, is greatest from July to September, inclusive, being then nearly three times as steep as from November to January. To the eastward quite marked contrasts, the strongest of the year, likewise exist between the summer pressures of Yuma and Fort Grant, Arizona.

The effect of this peculiar distribution of atmospheric pressure from July to September upon the weather conditions of the extreme western part of Texas is plainly evident from the observations of the prevailing direction of the wind at El Paso for three years, selected at hazard, which show that 24 per centum of the recorded directions of the wind from July to September, inclusive, the rainy months of the section, were from the east and 19 per centum more from the southeast or southwest. An examination of wind and pressure charts for the summer months of several years leaves no doubt as to the southerly atmospheric indraft on the eastern side of the Yuma barometric depression. The system of southerly winds inaugurated in this manner are more or less charged with aqueous vapor from the Gulf of California, which is situated within a moderate distance of southern Arizona, say from one to three hundred miles to the south or southwest.

In consequence of these conditions of pressure, wind, and humidity, there obtains over a great part of southeastern Arizona an abnormal rainfall during August, which exceeds on an average six inches over very considerable areas, and in favorable mountainous localities of Arizona and New Mexico even attains eight inches or more. From this great

August rainfall, which is exceeded in quantity in the United States only along the south Atlantic and east Gulf coast, the monthly precipitation diminishes gradually to the westward and is almost nothing near the center of the barometric depression in the lower Colorado valley. In like manner it decreases to the eastward until it amounts to less than two inches over the immediate Cis-Pecos region. The lightest rainfall for August in Texas falls on this debatable ground, over which unfavorable meteorological conditions reduce to a minimum the chances of precipitation, whether drawn from the Gulf of Mexico to the southeast or the Gulf of California to the southwest.

This condition of affairs is most favorable to western Texas, which would otherwise be an extremely arid region. The fact that the aqueous vapor around El Paso comes from the Gulf of California is also evidenced otherwise. In July the air along the coast of the Gulf of Mexico is charged with aqueous vapor to the extent of nine and a half grains on an average to each cubic foot of air. Following up the Rio Grande valley, the amount decreases from nine and four-tenths grains at Brownsville to about eight grains at Rio Grande City, seven grains at Eagle Pass, and six grains at the mouth of the Pecos. Up the Pecos valley the amount is only five and two-tenths grains at Fort Stockton, and to the westward seventy-five miles, at Fort Davis, as determined from three years' observations, it is five and one-tenth. But on going much further from the Gulf of Mexico to El Paso, which is twelve hundred feet lower than Fort Davis, the amount rises to five and nine-tenths grains. The much shorter distance from El Paso to the Gulf of California than to the Gulf of Mexico leads to the conclusion that the amount of aqueous vapor in the air during July to September also increases in the direction of the Pacific ocean. This conclusion is verified by the observations at Tucson, Arizona, where the mean for three years shows the following amounts of aqueous vapor in the air: July, 6.3 grains (0.4 grains more than at El Paso); August, the month of maximum rainfall, 6.8 grains, and September, 4.8 grains.

This extension eastward of rainfall, originally dependent for its source on an arm of the Pacific ocean, as far as western Texas, is a striking illustration of the fact, obvious with the precipitation over the temperate zone of the northern hemisphere with its westerly wind circulation, that precipitation is deposited with greater facility to the eastward of rather than to the westward of its origin.

The line of demarcation between the Trans-Mississippi type and that of the Gulf in eastern Texas and of the Trans-Pecos in western Texas cannot, in the nature of the case, be sharply and definitely drawn. In consequence, a very large part of the State, perhaps one-half, shares the combined characteristics of the three types whereby May and September are months of maxima (primary and secondary) rainfall and January and July months of minima (primary and secondary) precipitation.

It is very fortunate for Texas that neither the Gulf nor the Trans-Pecos type of rainfall distribution obtains over the whole State, since autumnal maxima of precipitation are not usually favorable to agricultural interests unless supplemented by abundant rain in other seasons. Happily the coast has sufficient rain in all seasons, save in years of exceptional droughts, while over the central meridians the obtaining maxima of the late spring are usually marked by sufficiently copious rains to subserve the needs of agriculturalists.

It thus results that the extreme western portions of the State are the only regions where deficient rainfall and unseasonable distribution make it regularly needful to resort to irrigation for successful farming.

In conclusion, it is not strictly relevant to the subject, but it may be a matter of interest to some of the Society to point out the bearing which the Trans-Pecos type of rainfall has upon the late experiment made under the supervision of the Department of Agriculture for the purpose of producing rain by artificial means. These experiments were made, I believe, either in the Trans-Pecos region or adjacent thereto,

in places where the Fort Davis rain-curve may be said to fairly represent the annual distribution of rain, as regards percentages. It was repeatedly asked why these experiments for producing rain were not made in the vicinity of Washington or at some point near the great centers of civilization, where facilities for experimental work were far greater than in Texas. Two reasons were assigned in the public press: the first, a most pertinent one, that the moral effect of success would be far greater if rain was produced in the regions to be directly and greatly benefited thereby. The second reason was most plausible but specious, it being to the effect that success would be inconclusive in the east, where rain conditions were favorable, but that in the west, under the well known unfavorable conditions, successful tests could be considered conclusive. It is not known that these reasons emanated from the official experimenters whose detailed reports have been long waited for, but so far in vain, by the public.

The diagrams herewith indicate clearly that the chances for rain in the Trans-Pecos region are by far greater in August than in any other month, being followed next by September; but it may pertinently be suggested that quantity of rainfall and the frequency of its occurrence do not always go hand in hand. This cannot be said, however, of the Trans-Pecos region, for from the charts published by the Signal Corps showing the probability of rainy days for August, as deduced from the observations of many years, it is ascertained that the percentages are as follows: Fort Elliott, 26; El Paso, 28; Fort Davis, 33; thus establishing the fact that, year in and year out, rain falls about every third day during the month of August. This would indicate that the officials in charge of the experiments, if harmless as doves, except to the public Treasury, evidently showed the wisdom of serpents in seeking the remote and scantily populated regions of western Texas as an appropriate ground for rain-making experiments during the months of August and September.

DIURNAL VARIATION OF THE BAROMETER.

BY

ALEXANDER SMYTH CHRISTIE.

[Read before the Society March 12, 1892.]

The moon and sun, attracting according to Newton's law, raise tides in the earth's atmosphere as well as in its tidal waters, and the periods of the former as well as of the latter tides depend upon the periods of terms in the Newtonian potential of the disturbing bodies. That these atmospheric tides have not been put more clearly in evidence must be due to the employment of defective and insufficient methods of search; or to their being in part transmuted, as fast as they are formed, into other species of motion, to subserve other ends in the economy of nature; or to both of these causes combined. I have no doubt that barometric records are obtained sufficiently precise to enable us to find these tides, using an appropriate analysis; but the only adequate analysis is the tidal harmonic. To indicate how grossly inadequate is a distribution according to the lunar day or half day, or to the solar day or half day, I may say that no one of these distributions provides for finding such important components as the lunar diurnal, solar diurnal, and luni-solar diurnal, which constitute almost wholly any diurnal tide arising from the attraction of moon and sun, a tide which must be relatively large at many barometric stations.

It may be further remarked that the part of the variation of the barometric column not directly due to the gravitational attraction of moon and sun depends, in one way or another, upon the rotation of the earth and the motions of

sun and moon. Suppose that this part of the variation is due to oscillations of the atmosphere set up by the thermal power of the sun: it must consist of at least two waves, whose fundamental periods are a solar day and a tropical year, respectively, corresponding to like thermal waves; and the relative and absolute magnitudes of these fluctuations render probable the formation of combination waves,* of which the two principal ones would coalesce with and reinforce the solar diurnal and luni-solar diurnal attractional; and in whatever other way the barometric variations depend upon the sun and moon, they will be resolvable into simple cosine waves which depend upon the earth's rotation and the motions of sun and moon; these cosine waves, with periods assigned *a priori*, being the natural and appropriate elements of the variations and readily picked up, if of sensible magnitude, by the methods of the tidal harmonic analysis.

It is also to be said that in so far as the terrestrial magnetic elements depend upon the tidal motions of the atmosphere,† or upon the action of the sun and moon as magnets, or upon the sun's thermal power, the appropriate analysis of observations of these elements is the tidal harmonic; and it is worth while to observe that if any part of the magnetic phenomena arises from the tidal action of sun and moon, as suggested by Professor Schuster in the paper cited in the note, the barometric variations dependent upon that tidal action will be correspondingly diminished and less easily detected.

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* Helmholtz, Berlin Monatsberichte, May, 1856, and Poggendorff's Annalen, vol. 99, pp. 497–540; or Wiss. Abhand., vol. 1, pp. 256–302; or his Lehre v. den Tonempfindungen (Ch. VII).

† I have considered this suggested dependence of the magnetic variations upon *tidal* motions only since reading the above, Professor H. A. Hazen having kindly referred me to Professor Schuster's important paper on the Diurnal Variation of Terrestrial Magnetism (Phil. Trans., vol. 180), which contains, *inter alia*, the conclusion that "the horizontal movements in the atmosphere which must accompany a tidal action of the sun or moon or any periodic variation of the barometer such as is actually observed would produce electric currents in the atmosphere having magnetic effects similar in character to the observed daily variation."

The interesting nature of these matters justifies an earnest recommendation that the tidal harmonic analysis be applied to the variations of the barometer and the fluctuations of the terrestrial magnetic elements. Should the tidal harmonic components be found in the barometric and magnetic variations a most important step will have been taken in the explanation and coördination of natural phenomena and an enchanting field of research will open before us.

GEOLOGY OF CHILHOWEE MOUNTAIN, IN TENNESSEE.

BY

ARTHUR KEITH.

[Read before the Society April 23, 1892, and published by permission of the Director of the U. S. Geological Survey.]

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The subject of the following paper is an episode in Appalachian history, the facts of which are recorded in Chilhowee mountain, in Tennessee.

The rocks of Chilhowee mountain are the oldest sediments in the Appalachians and on that account have particular interest.

The same formation occurs at intervals from Alabama to Newfoundland; hence the conditions that controlled its history have affected the entire Appalachian area.

I. Chilhowee mountain lies on the eastern edge of the great valley of Tennessee, about midway between Georgia and Virginia. Its wall-like front rises boldly from the smooth rolling valley and forms the western outlier of the Great Smoky mountains. Eastward between it and the Smokies lies a region of irregular peaks and ridges, relieved here and there by an open limestone valley or "cove."

(*a*) Chilhowee mountain is a straight ridge, with a sharp, narrow crest formed by hard layers of sandstone. It is quite even in profile, ranging from 2,000 to 3,000 feet in altitude; about midway is the Little River gap, the only one of note. Beside the main crest there are usually subordinate lines of

knobs and butts formed by the different resistant sandstone layers, and the watershed crosses frequently from one layer to another.

(*b*) On the east side of Chilhowee and near its center lies a narrow limestone valley called Millers cove. Its average width is one and a half miles and its length ten miles. To this valley special attention will be given in the following pages. As has been stated, it is the object of this paper to discuss the rocks of Chilhowee mountain and trace their connection with Appalachian history. The facts of the discussion are specially brought out by the limestone of Millers cove.

II. Professor James Safford, in his "Geology of Tennessee," called the Chilhowee rocks Potsdam, but made no subdivision of the formation. He did not discuss the limestone of Millers cove at all. No later publication has extended or controverted Mr. Safford's statements.

In 1889 the presence of Lower Cambrian fossils in the Chilhowee beds was first discovered by Mr. C. D. Walcott.

In 1889 the area came under my inspection in the course of areal mapping of formations under the U. S. Geological Survey. As the result of this preliminary survey, I announced the conclusion that the limestone of Millers cove and the Chilhowee sandstone were unconformable and separated by a long period of erosion. This was tentative, however, and only discussed among members of the survey. In 1890 I made a detailed survey of the region and confirmed the previous conclusion. Additional details were secured in 1891 by Mr. Bailey Willis and myself. The conclusion now appears well founded that after the deposition of the Chilhowee formation there came a period of erosion and then deposition of limestone over the eroded edges of Chilhowee.

III. The general geology of the region surrounding Chilhowee is as follows: On the northwest, the valley of Tennessee is eroded from a series of limestones and shales that range in age from Lower Cambrian to Carboniferous. These are folded in a series of narrow, parallel folds of great length,

usually closely squeezed and often broken. On the different parts of these folds the formations appear repeatedly in long, narrow bands, running northeast and southwest.

Southeast of this valley lies Chilhowee mountain, a long synclinal fold of sandstone and shale; part of its eastern border is formed by Millers cove.

From the Chilhowee group eastward through the entire Smoky mountain region is a great series of folded slates, sandstones, and conglomerates, the "Ocoee" of Safford, with the occasional quaquaversals of limestone coves.

IV. (*a*) The formations of the Tennessee valley are varied in lithologic form. The topmost in the section is the Carboniferous Newman limestone. Below that the Grainger shale and Chattanooga shale, of Devonian age. Then the Clinch sandstone, overlain in some places by Rockwood shale or "Dyestone" of Safford, of Upper Silurian age. Next below is the Lower Silurian Nashville formation, a variable combination of limestone, calcareous shale, sandy shale, and sandstone, 2,000 to 6,000 feet thick. This is preceded by Knox dolomite, Lower Silurian, a massive bed of magnesian limestone 3,500 feet thick. Below this come the Middle Cambrian beds in varying proportions of shale and limestone. They range in thickness from 600 to 3,000 feet. Under these lies the Middle Cambrian Rome sandstone, "Knox" of Safford, which is from 300 to 900 feet thick. Below this in places appears a bed of massive limestone, of Lower Cambrian age, from 200 to 700 feet thick. Lowest of all lies a series of sandy and argillaceous shales of uncertain thickness. They are Lower Cambrian and nearest to Chilhowee in age, but never come in contact with it.

(*b*) Areas of Chilhowee rocks are rare and only occur in isolated patches along the southeastern side of the Tennessee valley. As the Chilhowee mountain area is the only one where fossils have defined its age, its relations to later rocks there are of added interest. In this area more than in any other the deposit is well differentiated. Thus the facts of structure are more clearly defined, and inferences drawn from them are stronger.

The group of rocks called Chilhowee is an alternation of three beds of sandstone and three of shale. The shales only differ from each other in thickness, and all are fine, calcareous, and micaceous; on fresh outcrops they are blue, on weathered ones yellow. The upper bed is 300 feet thick, the middle 800, the lowest more than 1,000. The lowest only appears on anticlines and faults, and its total thickness is unknown. The sandstones differ somewhat, so that they can be distinguished from each other. The top bed is usually a fine, white sandstone or quartzite, of which 800 feet remain from erosion; the middle is a white sandstone, occasionally coarse, about 500 feet thick; the lowest has 700 feet of white sandstone, a thin bed of shale, and then 700 feet of conglomerate at bottom. The sandstone beds are composed of fine white sand and a few white quartz pebbles; the conglomerate has besides a large amount of feldspar in the form of pebbles and clay. These relations of material and thickness are quite constant over the entire length of the mountain.

(*c*) The slates of the Ocoee group, lying to the east, are similar to the Chilhowee shales when weathered, but are less calcareous, more argillaceous, and very much thicker. The Ocoee conglomerate is in places identical in appearance with the Chilhowee conglomerate, but usually contains less feldspathic material. Its coarse bottom bed, which specially resembles the Chilhowee conglomerate, is much thinner, averaging 400 feet, and is as variable in thickness and texture as the Chilhowee conglomerate is uniform.

d. (1) As has been stated, the structure of Chilhowee mountain is synclinal. The fold is usually open and its southeastern half is in places cut off by a fault. The synclinal axis of the mountain is also the axis of the cove, and the limestone there has the same fold as the Chilhowee beds. Both the syncline and these formations are bounded northeast and southwest by two thrust faults hading to the southeast. Owing to the uncertain amount of throw in these faults and their removal of the original contact relations, the rocks outside the faults can be assigned no precise relations

to the Chilhowee formation. Between the faults the only formation beside the sandstones and shales of Chilhowee is the limestone of Millers cove. Through this formation, therefore, must be determined the physical connection of the Chilhowee beds with succeeding formations, as it is the only one that retains its original contact relations.

(2) The Millers cove formation has two distinct parts, a limestone base and an upper red shale. The two members are closely associated, have suffered the same degree of folding, are parallel in dip and strike, and are interbedded at their contact. There is no break in their sequence and they are essentially of the same age.

(*a*) The lower member is a massive bed of 1,500 feet of gray-blue limestone, very cherty in its upper half. Three hundred feet from its top are two thin bands of green shale that aid in bringing out the structure. The upper cherty half of the limestone makes a line of high hills; the lower half a back valley mainly drained by sinks.

(*b*) The upper red shale member is composed of brilliantly colored shales mixed with thin sandstone at the top. The thickness remaining from denudation is about 1,500 feet. In topographic form its outcrop varies. From the upper sandstones result high rounded knobs; from the lower shales, valleys.

V. In order to have a satisfactory connection of the Chilhowee beds with later ones, the precise age of this limestone formation must be defined. The exact evidence of fossils has not been found, in spite of careful search by several members of the Geological Survey. Its age, therefore, must be determined by physical means for the present. For such determination there are available the lithologic character, thickness, and sequence in this limestone in comparison with like elements in other limestones of the province, the Knox dolomite and the Cambrian limestones.

(*a*) In thickness the limestone of Millers cove is midway between the Knox dolomite and the Cambrian limestone of adjacent regions. The Knox is usually 3,500 feet thick, and

varies from that but little along the eastern side of the valley. The Cambrian limestone in the valley near Chilhowee is made up of two beds, each about 400 feet thick, and parted by 200 feet of green shale. They diminish rapidly southwestward, and at the southwest end of Chilhowee mountain they are only 200 feet thick in all.

The cove limestone is 1,500 feet thick in each of the two deepest basins, and has in addition the closely associated red shale 1,500 feet thick. In view of the fact, to be discussed later, that the limestone rests on an uneven surface of erosion, it is quite possible that we do not have the full thickness anywhere exposed, but only the surplus over the height of the older surface. The deposit here may also be but the latter part of the equivalent formation in the valley, the earlier part being laid down at sea while this region was subaërial. On the score of thickness, therefore, the Cove limestone is more nearly equivalent to the Knox than to the Cambrian limestone. If the red shales are included with the Cove limestone, it fairly equals the Knox.

(*b*) In lithologic character there are no great differences between the Knox, Cambrian, and Cove limestone, because all the beds to be compared are limestones. The Cove limestone resembles both Knox dolomite and the Cambrian limestone, in that all are massive. The Cove limestone also contains many beds of silicious dolomite, such as give its name to the Knox dolomite, and these are entirely wanting in the Cambrian limestone. Both Cove limestone and Knox dolomite contain large blocks and nodules of black chert, which are extremely rare in the Cambrian limestone. In fact, the only cherty horizon beside the Knox is the Carboniferous limestone on the west side of the valley. In both the Cove limestone and Knox occur layers of limestone conglomerate. These are the result of special conditions of erosion during the deposition of the dolomite, and the same special result in the Cove limestone is of great significance in indicating equivalence.

(*c*) The red sandy shales associated with the limestone in

Millers cove are of a type often found in Tennessee. They resemble minutely the Rome sandstone (Knox of Safford), the Silurian Rockwood shale (Dyestone of Safford), and a bed in the Devonian Grainger shale west of Chilhowee. They also resemble as strongly the red shales near Guide mountain and the red shales along Watauga river. In both the latter places the red shales are replacements of Knox dolomite, due to the proximity of shore during their deposition. At Guide mountain their equivalence is fairly well shown and their age is defined by the Middle Cambrian shales 200 feet below them. Along Watauga river the geographic relation to shore is well shown, as well as the geological fact of replacement of limestone by shale. The red shale increases toward shore until in places it entirely supplants the limestone. The abundance of ripple-marks demonstrates the shallowness of the water at the time of deposition, and the identity of conditions in the Miller's cove and Watauga areas is suggestive—*i. e.*, quiet deposition of limestone locally affected by sand from neighboring lower Cambrian shores, and it has considerable weight in identifying the resultant formations.

The presence of chert in the red shales replacing the Knox is the chief difference between them and other red shales; in the latter no chert occurs. Thus the chert, which is a constant feature of the Knox in spite of shore variations, is seen to be the chief lithologic feature of that horizon, and it must indicate a general condition during deposition. Its limitation to Knox and its occurrence in the Millers cove limestone, therefore, are good evidence that the latter is Knox.

(*d*) The only remaining basis of comparison between the Cove limestone and other limestones—that of sequence—is limited by the small range of sequence in Millers cove. The deposit there has two members, cherty limestone and red shale, following an unconformity. This sequence occurs in part in the Lower and Middle Cambrian where the Rome sandstone follows a limestone. There, however, the lime-

stone is very much thinner than in Millers cove, is not cherty, and does not follow an unconformity. The Rome sandstone, in its nearer outcrops, is less than half as thick as the Millers cove shale, is not cherty, and is much more sandy. The only other limestone, the middle Cambrian, is not followed by any red shale and hence does not furnish the same sequence. The dolomite, however, shows such a sequence wherever the shore was near at hand, in the cherty limestone and red shale following an unconformity.

(*e*) It has been stated that no fossils have been seen in the Millers cove series. This paucity of life serves as an additional distinction between the Cambrian beds and the Millers cove rocks, because fossils can readily be found everywhere in the Cambrian beds. The barrenness of the cove series is paralleled in the dolomite, which has scarcely a fossil except at the top and bottom. This evidence is negative, of course, but has some value, in that it corroborates other evidence.

(*f*) To sum up, the following points of difference and resemblance appear between the Millers cove series and other beds: The Cove limestone and the Knox dolomite are the only cherty deposits before the Carboniferous. They have the only beds of white dolomitic limestone. They have limestone conglomerate beds, the mark of a special erosion condition. The Cove limestone is thicker than any except the Knox, and may be equally thick in full presentation. In the Cove limestone and Knox are developed the same phase of red cherty shale under similar shore conditions. They have the same sequence, and it is the only parallel one. They contain practically no organic remains, and are the only beds so specially marked.

These are indications of identity of Knox dolomite and Cove limestone. Opposed to them are the negative points, that the Cove limestone is not known to be as thick as the Knox, and that there are limestones and red shales other than Knox that might be equivalent to the Cove series. The first objection has been discussed and has small weight.

The second, that of lithologic similarity, is of very little value when unsupported, as it is here. The evidence seems decidedly to favor the identity of the Knox and Cove limestone.

VI. (*a*) In the preceding pages the discussion has turned on the relations of the Cove limestone to similar rocks outside the faults of Chilhowee, in order to discover the age of the Cove limestone. The knowledge of this is, of course, essential to a final application to Appalachian history of the contact relations of the limestone and the Chilhowee group. It is the belief of the author that the evidence fully warrants correlation of Knox dolomite and the Cove limestone. In the rest of the paper the exact nature of the contact relations will be considered and the results applied to the larger field of the Appalachians.

The most interesting feature of the geology of Millers cove is the contact between the limestone and the Chilhowee formation. This is represented on the accompanying map by a heavy black line, and follows the outline of the cove quite closely. On casual examination the sequence through the Chilhowee beds and limestone seems to be unbroken, but the details of the contact show wide differences in adjacent sections. It is not always the same part of the Chilhowee sandstone that touches the limestone or the same part of the limestone that touches Chilhowee. This feature appears plainly in the accompanying map and sections. The position of each section on the map is indicated by black lines lettered to correspond with the sections. The longitudinal section K is derived from sections A to H. Section M is derived from section K by reducing the folds shown in the limestone to a level.

In the limestone the position of the contact has a vertical range of 1,200 feet; in the sandstone, of 1,000 feet. In a general way these differences complement each other, so that where the limestone is thinnest the sandstone is thickest, and *vice versa*. In fact, the limestone and part of the sandstone appear to have the same vertical horizon and to replace each other.

This vertical relation may have been due to any one of three causes: (*a*) contemporaneous deposition of limestone and sandstone in adjacent areas; (*b*) a thrust fault bringing the limestone over the sandstone and traversing the beds of both; (*c*) deposition of limestone over an irregular basin in the sandstone.

(1) In favor of the first hypothesis, that of contemporaneous deposition, may be cited two apparent transitions between limestone and sandstone. The first, on the line of section B, is quite peculiar. The limestone there is very silicious, with both secondary quartz and rolled quartz grains; it occupies a narrow ravine between two sandstone knobs, and is directly in the strike of the sandstone masses.

(2) The second, near the line of section G, is apparently a vertical passage from sandstone through sandy limestone into massive limestone. At the contact of sandy limestone and sandstone there is a difference of 60° in strike between the two, and a small bed of black, calcareous shale fills in the irregularities of the sandstone. This slate is associated with the limestone by its calcareous nature, and contains nodules of limestone and fragments of sandstone as large as the fist. The sandstone pebbles are grayish white, fine-grained, and similar to the Chilhowee sandstones. There is no known formation previous to the dolomite whence they could be derived except Chilhowee. These pebbles prove that the slate and overlying limestone came after the sandstone, and that some of the sandstone was subaërial at that time. The slate bed proves an interruption and change in deposition—*i. e.*, that there was no transition.

Comparison of the two contacts with reference to the precise horizon of the limestone and sandstone in contact brings out the further fact, which cannot be reconciled with similtaneous deposition—*i. e.*, the limestone touches the middle sandstone of Chilhowee in section B and the upper one near section G. The range of the contact between these horizons in the Chilhowee series is 700 feet.

(3) Even greater is the distance in sections C and G. In

C 200 feet of limestone rest on the middle sandstone; south of G 1,200 feet of limestone rest on the upper quartzite. Thus there is a range of contact of 1,500 feet between the two sections—500 feet in the Chilhowee beds and 1,000 feet in the limestone.

(4) At the southwest end of the cove the limestone dips beneath the surface and the red shale is in contact with the upper quartzite. Here, therefore, the limestone is lower than the quartzite. The reverse is shown in the northeast end of the cove, where the limestone lies above the quartzite and dips away from it.

(5) At Little River Gap the two series are discordant in strike by 50° and in dip by 40° for a considerable distance.

Excepting the two outcrops cited, there are no evidences of transition between Chilhowee and the limestone. Each formation is uniform up to the contact line, and the boundaries are sharp.

(6) As the limestone and red shale are an interbedded series, the equivalent of one is the equivalent of the other also. In considering, therefore, the deposit of limestone and Chilhowee as contemporaneous, the red shale must be accounted for. The red shale is nearly as distinct from the Chilhowee shales as the limestone and has no equivalent in the Chilhowee formation. A few thin layers of red shale occur, but they are in the bottom bed of Chilhowee, the conglomerate, and far below the limestone. The 1,000 feet of red shale in the cove are ripple-marked mud deposits, while the Chilhowee sandstones are clean and pure offshore deposits. It scarcely seems possible that such diverse beds should coëxist within half a mile without modifying each other.

(7) Finally, the thickness of limestone, 1,500 feet, exceeds that of the eroded sandstone, 1,200 feet. As limestone is a rock of slow accumulation, probably two or three times as slow as sandstone, the time needed to deposit this 1,500 feet of limestone is very much greater than that needed to deposit the sandstone. In fact, the period required for the 1,000 feet

of red shale alone is enough to cover the time for the sandstone deposit. Thus the hypothesis of simultaneous deposition must harmonize the following unique conditions: Accumulation of 2,500 feet of slowly forming limestone and shallow-water shale contemporaneously with 1,200 feet of quickly forming sandstone, in sharply defined and contiguous areas.

(8) To sum up, the limestone lies here higher and there lower than the upper quartzite. It is thicker than the quartzite it seems to replace, although of slower accumulation. It is discordant with the Chilhowee group in strike and dip. It is lithologically distinct from Chilhowee and only twice shows traces of a transition bed, which may also be regarded as a basal sandstone. These apparent transitions take place between different horizons in both limestone and quartzite. The limestone series has no possible equivalent in Chilhowee for its red shales. Lastly, at one contact the limestone series contains fragments of quartzite. On these grounds it seems necessary to exclude the first hypothesis, contemporaneous deposition, and test the second and third.

VI. (*b*) The second hypothesis, that of discordance produced by a thrust fault, is next in consideration. The contact of dolomite and Chilhowee resembles a fault plane as far as variety of beds in contact is concerned, but the changes are even more abrupt than is common with faults. Section B, before described, is an instance of this; the filling of a narrow ravine by limestone without change of strike and dip on either side would be a very unusual feature in a fault.

(1) In one important point the plane of discordance differs from a fault plane. It is folded in with the formations as if it were a bedding plane, and has endured the full extent of deformation that they have, even to overturning. Ordinary types of fault are displayed in the two bounding the Chilhowee area, and their connection with the synclines is obvious. They are a later stage of deformation, and, rising out of well developed anticlines, ride over the synclines and in places cover them completely. Few fault planes in the

Appalachians have been distorted after their production, and in them the folding is very rarely great. Here, however, it is carried even to overturning, as shown south of section G. As this fold has the full amount of Appalachian deformation and ordinary faults were developed during its later stages, then this plane of discordance must have preceded the recognized Appalachian folding, if it be a fault, and have resulted from a previous period of deformation. Hence a special period of disturbance must be assumed, if this discordance be considered a fault, and of this new period we have no other evidence.

(2) Another difference between fault planes and this contact is illustrated in the transition previously described south of section G. In that locality the bottom bed of the limestone series, a calcareous slate, contains fragments of the quartzite. The same feature appears on the opposite or northwest side of the cove, where there are outcrops of a breccia of sandstone and chert at the contact of the limestone and the Chilhowee sandstone.

(3) To sum up, the mere discordance along the plane of contact is common to the three hypotheses and decisive of neither. The well developed folding of the plane, if it is a fault, necessitates the assumption of a new and unrecorded period of deformation. The irregularities of the plane exceed those of faults. The fragments of sandstone can only be introduced into the limestone during the process of sedimentation. For these reasons the hypothesis of faulting seems inadequate to explain the discordance.

VI. (*c*) The hypothesis of erosion and unconformable deposition remains, therefore, as the only explanation of the observed discordance. Negatively it is the explanation, because other possible ones are inadequate; positively it is, because certain features of the contact can only be produced by sedimentation. These features are the inclusion of fragments of lower rocks in beds above the plane of discordance and the presence of limestone in ravines in the Chilhowee sandstone. This explanation is corroborated by the existence of uncon-

formity at the same horizon in other places. The instances of this will be discussed later in detail.

VII. The unconformity being accepted as a fact, at once several questions are raised. If the Chilhowee surface beneath the limestone is one of erosion, then the Chilhowee beds emerged after deposition and were exposed to subaërial forces. How extensive was the erosion in duration, depth, and area? Was the emergence local or wide-spreading? Was the elevation that caused the emergence a simple, continental one, or elevation combined with folding? These questions can be answered partly from the sections in Millers cove and partly from study of the Cambrian formations in the Tennessee valley.

(*a*) The maximum visible removal of Chilhowee beds is 1,100 feet, and this occurred in two places, sections B and F. Of course this is only the actual overlap visible in the Cove and would be increased 500 feet by a moderate estimate for recent erosion below the Chilhowee-dolomite plane outside of the cove. Such a deep erosion represents a considerable lapse of time, even under the most rapid degradation.

b. (1) The question of the geographic extent of Chilhowee erosion is not easily answered. In Millers cove a basin was excavated twelve miles long and 1,100 feet deep. The surrounding higher land was therefore at least 1,100 feet high at the end of erosion. Such an uplift, if of the continental type, would have laid bare a very large area. If the uplift was combined with folding, then the areas of Chilhowee rocks exposed might have been small and the question of emergence or non-emergence must be determined for each area. *A priori*, an elevation of 1,100 feet would expose a considerable area to the air, even if the beds were somewhat folded.

There is evidence favoring both possibilities. In Millers cove folds were made before the limestone was deposited, as shown in section M. This section is drawn on the assumption that the beds of limestone were deposited in horizontal layers. This is probably not far wrong; and even if they

dipped toward the center of the basin when originally formed, then their restoration to a horizontal position would diminish the original depth of the basin.

There appear in the section two synclines with dips from ten to thirty degrees and a length of twelve miles, which is a considerable amount of deformation. It may have been a local manifestation, however, such as are found in later beds in isolated places.

(2) No other area has been studied in such detail as this, and the evidence at hand is conflicting. In Dennys mountain, twenty miles northeast of Chilhowee mountain, the Knox lies in one place conformably on the Chilhowee sandstone, in another abuts directly against it. The structure is too complex, however, to warrant very exact inferences. Northeast from Dennys mountain the Knox-Chilhowee contact is made by a fault.

(3) Iron and Holston mountains, near the Virginia line, have been but little studied, but their structure is so simple that inferences from it are reasonably strong. These mountains contain a wide syncline of Chilhowee rocks, dipping ten degrees, and on it the Knox lies conformably.

(4) Southwestward from Holston mountain, near the Nolichucky river, the Chilhowee beds in Iron mountain thin materially and nearly disappear between Knox and granite. This may be due to erosion of the Chilhowee before the Knox or to original thinning and overlap of the Chilhowee near its shore.

(5) Southeast of Iron mountain, in Hampton valley, the Chilhowee beds thin from 2,000 feet to 100 feet in three miles between Knox and granite. This, too, may represent either erosion or overlap, as far as is now known.

(6) In Limestone cove a central area of Knox is surrounded by a ring of granite, and the whole valley by a high wall of Chilhowee sandstone. The Chilhowee beds form an anticline with the cove in the center. Chilhowee was first deposited on the granite, then Knox. As there is no Chilhowee sandstone here between Knox and granite, at least 2,000 feet

of Chilhowee must have been removed before deposition of the Knox took place. It is most likely that the removal was due to erosion, as the Chilhowee surrounds the cove and there is no evidence of a fault, least of all a circular fault. If Chilhowee sandstone was eroded there, it is likely that the present anticline was an original one and furnished the locus for erosion of the pre-Knox basin.

(7) Although these areas do not prove post-Chilhowee erosion, they do present a strong probability of it. The salient fact in them is the conjunction of Knox dolomite and Chilhowee sandstone without the Cambrian beds that precede Knox in the valley. This contact continues northeast into Virginia for at least seventy-five miles, with an overlap of all the later Cambrian beds. At Balcony Falls, Virginia, Middle Cambrian comes between Chilhowee and Knox. Whether this overlap could be simply a lapse of time, with neither deposition nor erosion, is a difficult question to answer. Such conditions appear to occur among later sediments at several horizons, but they are mainly in off-shore or quiet water formations and not near shore as these are. It certainly would require for its consummation either a suspension of ordinary conditions or a very delicate balance of the upper beds near water level. In any event, the existence of erosion is not precluded, though it is not very probable.

From the *possibility* of erosion in most sections, its *probability* in many sections, and its *existence* in Millers cove, the inference is warranted that erosion occupied the interval between Knox and Chilhowee.

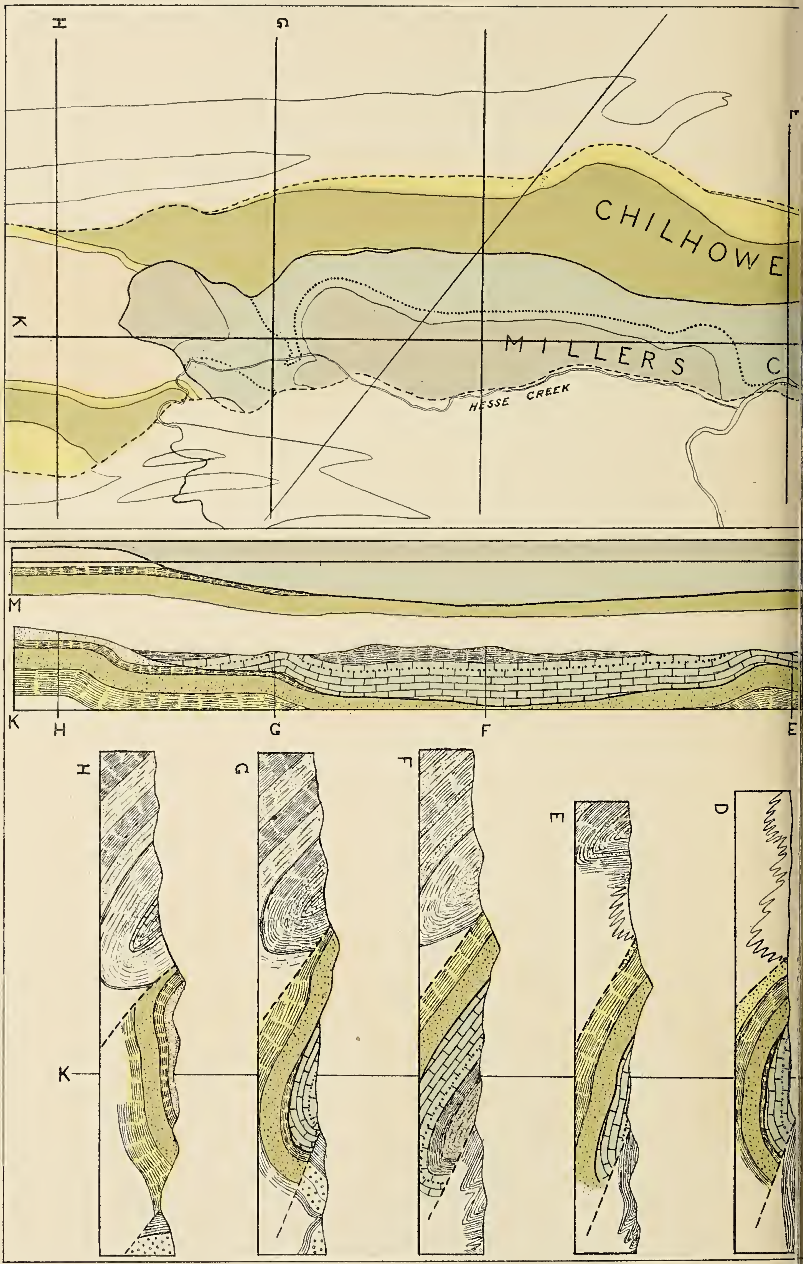
VII. (*c*) The question of the extent of the pre-Knox deformation of the Chilhowee beds is even more uncertain. Such deformation seems to exist in Limestone cove, but not in Holston mountain. In Dennys mountain it appears to be present in one place, though absent in most. In Millers cove it did exist and in sufficient amount to show a strong compressive force.

The forces that produce folds may be assumed to be general in their nature, as their evidence is general and universal.

These scattered instances of folding were, in all probability, the local reliefs of strain at early stages of a general compression, and similar small folds may be expected elsewhere. It appears plain that there was no general deformation before the deposition of Knox dolomite, and that the movement was mainly a simple uplift.

VIII. The date of the post-Chilhowee unconformity in Millers cove depends on the age of the limestone there, as has been said. Independent of that, however, two facts stand out—erosion of Chilhowee sandstone at some date and absence of later Cambrian beds on the Chilhowee. If the two are the same, then the age of the erosion interval is well defined; if they are not, then the Chilhowee-Knox interval, a single one along most of the shore, was interrupted at Millers cove by a long period of quiet deposition.

From the evidence herein discussed it is probable that the interval was a single one along the whole Tennessee shore and far into Virginia. The announcement of this interval is new, I believe. It certainly is new in that it ascribes the interval to erosion and deformation, and it opens an entirely new page in Appalachian geology. By it the disturbances that culminated in Appalachian folding and uplift are shown to have begun after the deposition of the *first* Palæzoic beds instead of the *last*, as usually supposed.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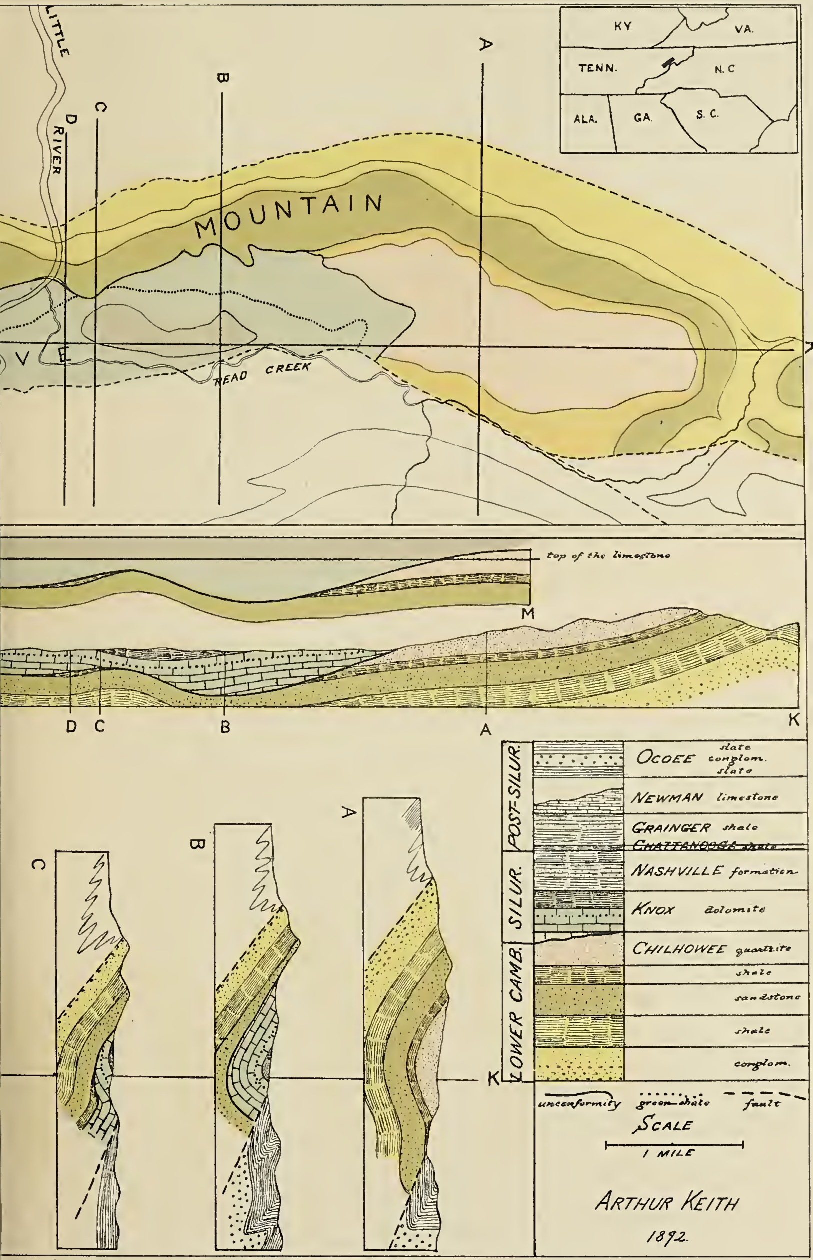

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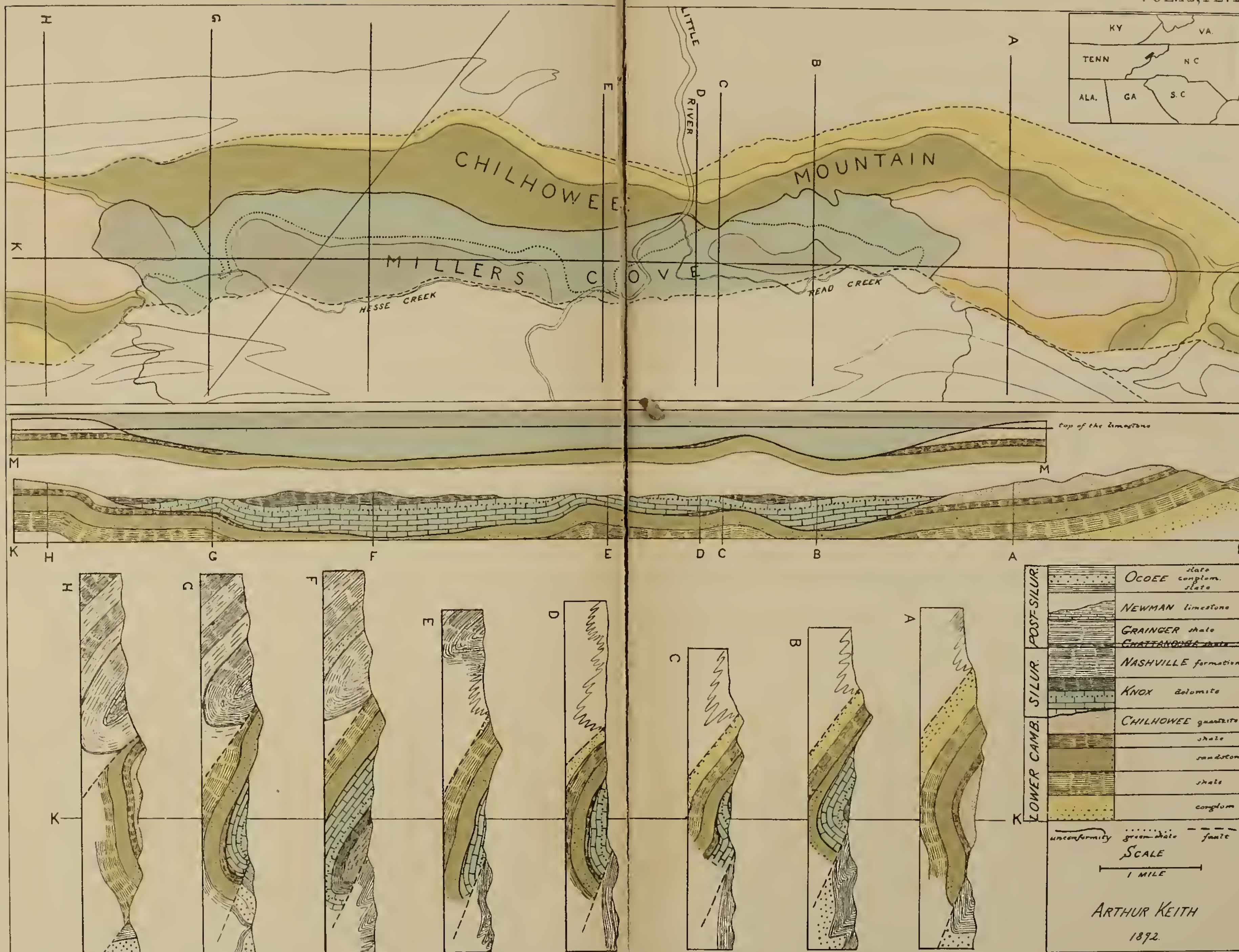

KEITH. GEOLOGY OF CHILHOWEE MOUNTAIN.

THE ORIGIN OF IGNEOUS ROCKS.

BY

JOSEPH PAXSON IDDINGS.

[Read before the Society May 7, 1892, and published by permission of the Director of the U. S. Geological Survey.]

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INTRODUCTION.

The object of the present paper is to give the writer's reasons for concluding that all of the volcanic and other igneous rocks of any region are so intimately connected together by mineralogical and chemical relations that they must have originated from some single magma, whose composition may be different in different regions; and, further, that it is the chemical differentiation of this primary magma which has given rise to the various kinds of igneous rocks.

It is not intended to discuss in this place the physics of the interior of the earth, nor the possible cause of volcanic activity; but the attempt will be made to present more fully than has heretofore been done the petrographical, chemical, and geological data bearing upon the proposition just stated.

It will be advisable, however, before entering upon this undertaking to review the opinions of some of the most prominent writers of recent times with regard to this question; for "It should be borne in mind," wrote Von Richthofen in 1868,* "that among the theories recently proposed upon the subjects specified above [namely, the nature, origin, and mode of eruption of igneous rocks], there is not one which has not already had its prototype in the phantasmagoria of the time of the dawn of geological science, and that it is these which have been constantly reproduced, enlarged, diversified, remodeled according to the advance of science, and supported by continuous accumulation of evidence."

It will be seen in the sequel that the writer is only contributing to the establishment of one of the theories which was originally conceived with the utmost crudeness, and has been gradually assuming greater definiteness and perfection.

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* Von Richthofen (F.) "The Natural System of Volcanic Rocks." 4°. San Francisco, 1868, p. 47.

HISTORICAL REVIEW.

Among the earliest views regarding the nature and origin of volcanic rocks the most prominent conceived the various molten lavas to have existed as such from the first consolidation of the earth—that is, to belong to the originally fused matter of the globe—while another class of theories considered them to be fused portions of metamorphic or sedimentary rocks. Without entering into the earliest of these theories, we may begin our review by mentioning the first expressions of Scrope * on the variations of volcanic rocks, made in 1825, in his work on "Volcanos." It is to be remarked that at that time Scrope considered volcanic rocks or lavas as erupted forms of previously crystallized rocks, which were rendered fluid by "intumescence," the cracks between the mineral crystals being filled with water which, when highly heated, gave fluidity to the whole mass. The rocks themselves were not considered to have been actually molten in most cases at the time of eruption.

This explanation is necessary to an understanding of his views regarding "the circumstances which may have given rise to the variety of mineral composition observable in the lavas."

"This variety," he says, "must be supposed, owing to one or other of two causes, viz:

1. "An original difference in the nature of the subterranean crystalline rock, whose extravasation produced them on the surface of the earth; or,

2. "To changes produced in this rock, originally of an uniform composition, during the process of elevation, which was probably accompanied in many instances by repeated intumescence and reconsolidation before the final emission of the lava.

"I own that I incline to the latter alternative, although I am far from being able to explain the mode in which every such change can have taken place.

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*Scrope (G. P.) Volcanos. 8°. London, 1825, pp. 21, 25, 26.

"Our knowledge, indeed, of the laws which determine the formation of different minerals from their elementary ingredients is at present too limited for the solution of such a question."

Having admitted his lack of knowledge on this subject, he proceeds to construct a very remarkable hypothesis, by which he expects to obtain trachyte and basalt from a granite, and which is here quoted in full, because he refers to it in a subsequent edition of his work on volcanoes.

"It would not, indeed, be difficult," he says, "to conceive the production of ordinary trachyte, by a very slight change, from a granitic original; for the process of intumescence, when carried far, may easily be supposed to change the feldspar crystals from compact to glassy; to dissolve the whole or the greater part of the quartz in the aqueous vehicle, forcing it to assume the crystalline form of feldspar on consolidation (as in graphic granite); finally, to volatilize the mica, of which part would recrystallize in more perfect crystals, either of mica or augite, on cooling; and a part, perhaps, give rise to specular or magnetic iron disseminated through the rock or lining its pores and fissures.

"In like manner we may imagine the production of basalt to have been caused by the exposure within the vent of a volcano of an intumescent mass of granite to reconsolidation, effected by the augmentation of temperature and consequent expansion of its lower beds. In these parts the extreme heat may be supposed to volatilize the mica and other ferruginous minerals, while the intense pressure would separate them in a gaseous state from the feldspar, thus leaving a feldspathose lava, with very little iron, in one part of the chimney and occasioning the crystallization of a highly ferruginous lava in another.

"The subsequent intumescence and protrusion of these lavas might produce alternate currents of trachyte, clinkstone, or compact feldspar, and basalt or graystone."

With reference to the occurrence of trachyte and basalt, Scrope maintained, in opposition to Humboldt and Beudant,

that there was no evidence of repulsion between these two rocks, "the production of trachyte preventing that of basalt, and *vice versa;*" but "the general law seems to be that they occur together, being produced successively from the same or proximate vents, though, in general, at long intervals of time." Scrope thus advocated the view that the various lavas originated from a common source, which was a pre-existing crystalline rock, by a process of differentiation caused by the volatilization of the ferruginous minerals and their separation in a gaseous state from the feldspathic portion by pressure.

In 1844 Charles Darwin * published his observations on Volcanic Islands, and expressed his views as to the origin of igneous rocks. He appears to have been led to his position by observing in certain instances that the porphyritical crystals in some lavas have settled to the bottom of the lava-sheet, and he cites the case of certain obsidians in which this is said to have taken place. He therefore considers the consequence of the partial crystallization of minerals and their settling because of their specific gravity, and suggests that this explains why, as he believes, basalts generally break out at the base of volcanoes and obsidians from their summits. In applying this theory to plutonic rocks he considers greenstones and basalts as having separated out from earlier rocks, and mentions the occurrence of dikes of these rocks in granite regions, and asks the question, Whence have these dikes come? Are we to suppose a substratum of trap beneath the granitic series? "Is it not more probable that these dikes have been formed by fissures penetrating into partially cooled rocks of the granitic and metamorphic series, and by their more fluid parts, consisting chiefly of hornblende, oozing out and being sucked into such fissures?" As an illustration of this, he describes an instance in which he observed thin threads of dark rock penetrating the gneiss on either side of dikes of a dark-colored augitic or hornblendic rock, and states that he considers these to have been the "feeders" of

* Darwin (C.) Volcanic Islands. 8°. London, 1844, pp. 117–124.

the dikes, and not thin offshoots from the dikes into the gneiss.

This view of the origin of trap dikes being admitted, he continues: "We may further admit in the case of a great body of plutonic rock being impelled by repeated movements into the axis of a mountain chain, that its more liquid constituent parts might drain into deep and unseen abysses, afterwards, perhaps, to be brought to the surface under the form either of injected masses of greenstone and augite-porphyry or of basaltic eruptions."

Darwin has thus formulated two hypotheses—one based on a partial crystallization and separation by settling on account of differences of specific gravity between the crystals and the mother-liquor, and the other based on the partial crystallization of a magma and the oozing or squeezing out of uncrystallized portions. Both consider the different varieties of eruptive rocks to have originated by the separation of some primitive homogeneous mass.

In 1849 Dana, in his report as geologist on the Wilkes Exploring Expedition,* discusses the mineral constitution of the basaltic islands of the Pacific. He observes that the volcanic rocks of these islands vary "between basalt and clinkstone or porphyry, the former passing into the latter as feldspar becomes the predominant and finally the constituent mineral." The rocks grade from glassy to "a variety of crystalline texture like syenite." In seeking "the origin of these different rocks and the peculiar conditions and relations under which they occur," he calls attention to the fact "of the occurrence of feldspathic varieties at the center of the mountains, while the exterior and circumferential portions consist of basaltic rocks," and asks, "What has separated the feldspar, iron, and augite that constitute the basaltic rocks and left nearely pure feldspar alone at the center"?

The difference in the specific gravity and fusibility of

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*Dana (J. D.) United States Exploring Expedition during the years 1838–1842, under the command of Charles Wilkes, U. S. N. 4°. Philadelphia, 1849. Vol. 10, Geology, p. 372 *et seq.*

augite and feldspar and of basalts and feldspathic rocks is pointed out, and the ground is taken that supposed differences of age can have had no influence on the character of the rocks, " for heat and pressure and slowness of cooling, the requisite material being present, are the prominent influencing causes determining the formation of particular minerals or rocks," and the requisite material is shown to have existed from ancient times.

" These feldspathic rocks are apparently an essential part of the volcanic dome during its whole progress, as they occur along the center, often to the very summit." He does not think they were inserted after the dome was completed, nor thrown up before hand, but concludes that " the main body of the mountain and its feldspathic center were in contemporaneous progress at least as long as there were summit eruptions."

As to the view presented by Darwin, that a separation of the basaltic material by gravity may have taken place, he argues that Darwin supposes the feldspars and augites rise or sink as crystals of these minerals; " but if anything is in fusion it is feldspar or augite (supposing these to be the constituents of the lava), and while in fusion there are no crystals, as crystallization is the first step in the process of solidification. If there is any sinking, therefore, it must be a sinking and separation of these materials in the fluid state, or, at least, this must be the case with the augite, which is the more fusible of the two minerals, and will be the case with both at certain depths, wherever the temperature is that of the fusion of feldspar. The impossibility of there being any crystals of feldspar will farther appear from the fact that the material of clinkstone is generally without a distinct crystalline texture." He also shows that the eruptions from the summit of Kilauea have a high specific gravity, which would controvert Darwin's hypothesis.

The explanation offered by Dana is based on the presence of vaporizable substances and the unequal fusibility of feldspar and augite.

The presence of vapors as the molten magmas approach the surface inflates them and lowers the specific gravity. His hypothesis hinges on the idea that "wherever the temperature of the liquid mass begins to be less than that necessary to retain the feldspar in fusion, there the feldspar will commence to solidify, or will slowly stiffen in the midst of the fluid material made up of the other ingredients. In this state the vapors ascending in the conduit will urge upward the feldspar much less freely than the more liquid part of the lava."

He adds "that it does not necessarily follow that the lavas of fissure eruptions have, in all instances, been thus derived," for they may proceed from portions of the liquid lavas which have not experienced this separating process, which only takes place where the vapors have a chance to ascend and escape.

He questions whether the mineral, augite, exists in a molten state as a fixed compound or in some different combination, and considers it possible that "the magnesia of the lava, or a large part of it, is engaged in combination and used up, as we may say, deep below," in the formation of olivine. Whether it be wholly or partially used up would probably depend on "the proportional amount of silica present and the temperature, the rest, at a higher elevation in the lava conduit, going to the formation of augite or some allied compound."

As to the production of syenite he remarks: "The feldspathic center is enclosed within a thick covering of rocks, and will therefore cool slowly; and, though generally forming only disseminated crystals of feldspar in an earthy base, the cooling is sometimes sufficiently gradual to allow of the whole crystallizing, and in this case the texture throughout is crystalline and the rock much resembles a granite. Under the same circumstances (or even a less gradual cooling) the elements of augite present will crystallize as hornblende." * * * "The same remarks will also apply to mica," while "the excess of quartz not in combination" will behave like the feldspar.

Thus he concludes "that particular rocks have no necessary relation to time on our globe." * * * "It requires only different circumstances as regards pressure, heat, and slowness of cooling to form any igneous rock the world contains."

In 1851 Bunsen,* after a visit to Iceland and a chemical investigation of the volcanic rocks occurring there, arrived at the conclusion that any of the varieties of these rocks could be considered as a mixture of two extreme magmas, one of which represented the most basic and the other the most acid form of the volcanic rocks of that region. His own calculations and comparisons show that he did not require a very great degree of accord between the observed proportions of the various constituent oxides and those calculated from his hypothetical extremes, even after he had combined the alumina and iron oxide, constituents which in the mineral composition of rocks have little or no connection with one another. He suggests the theory that all of these volcanic rocks were derived from two reservoirs of magma, one the basic or "normal pyroxenic" and the other the acid or "normal trachytic," either separately or in combination, the two magmas commingling in various proportions to form the intermediate varieties. He seems to have viewed the question from a purely chemical standpoint, regardless of the geological or physical difficulties to be overcome. The simplicity of the hypothesis and the unquestionable chemical relationship between the volcanic rocks of this and of other regions, which was thus brought to light for the first time, gave a force to Bunsen's theory that impressed it deeply upon the science, though for geological reasons it was very soon discredited. We are indebted to Bunsen, however, for calling attention to the chemical relationship of igneous rocks and initiating a line of investigation which is at present far from being completed.

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*Bunsen (R.) Ueber die Processe der vulkanischen Gesteinsbildungen Islands. Pogg. Ann. d. Phy. u. Chem. 8°. Leipzig, 1851, vol. 83, No. 6, pp. 197–272.

Bunsen's theory was not long permitted to go unassailed. Two years later, in 1853, Sartorius von Waltershausen published the results of his studies of the volcanic rocks of Etna and Iceland.* In this work he combined the chemical study of the essential minerals of rocks with certain physical conditions of the earth, and what were then considered by some to be well-established geological facts. The chemical portion of the theory was grounded very largely on the gradation in composition and specific gravity of the feldspar series, which was made to include a species called krablite, with from 74 to 80 per cent. of silica; so that albite and orthoclase occurred in the middle of the series instead of at the most silicious end, as at present, since krablite has been found to be a rock. A consideration of the mean specific gravity of the earth and that of the elements forming its crust without allowing for a possible influence of pressure on the material within the globe led him to conceive of the earth as being made up of concentric shells of variable specific gravity, increasing from that of quartz at the surface to that of bismuth and silver at the center. It was also thought to have been established that the earliest igneous rocks which had been erupted in past geological ages were highly feldspathic and silicious ones, allied to granites, and that more recent ones were less silicious, and the most recent lavas the most basic; and the gradual cooling of the globe having consolidated the molten interior at gradually increasing depths, the earlier eruptions would have come from nearer the surface than the later ones.

These considerations led Von Waltershausen to the theory that "the whole exterior crust of the earth is to be considered as having been originally a feldspar mass, which contained at the surface relatively very few foreign mineral bodies, but which came to an end at a depth of about 94½ miles, after it had reached its most basic composition, and was followed by specifically heavier silicates and metal oxides, especially by

* Von Waltershausen (S.) Uber die vulkanischen Gesteine in Sicilien und Island, etc. 8°. Göttingen, 1853.

augite and magnetite." .* * * "If one considers the structure of the crust of our earth to be fashioned in this manner, one soon arrives at the decided opinion that all of these crystalline rocks form a single, continuous chain, whose members, intimately interwoven with one another, commence with the oldest granitic formations and end with the most recent lavas." *

Concerning Bunsen's theory, he says that in order to explain the manifold variations of mineral and chemical composition of igneous rocks, Bunsen, having overlooked the simplest and most natural hypothesis—the disposition of matter within the earth according to an increase of density from the surface inward—seized upon a very improbable hypothesis of two separated volcanic reservoirs, which contain acid and basic silicates, through whose mixture or fusion together the innumerable gradations of rocks have been brought about. This hypothesis may be accepted as a piece of pure fiction; but it must not be supposed that it is grounded in nature or that two separate reservoirs containing basic and acid fluids actually exist within a volcano.†

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* Op. cit., p. 528:

"Die ganze äussere Erdrinde ist in primitiver Form als eine Feldspathmasse zu betrachten, die an der Oberfläche nur verhältnissmässig wenige fremde Mineralkörper enthält, die aber, nachdem sie ihre basischste Zusammensetzung erlangt hat, in einer Tiefe von etwa 21 Meilen aufhört und durch specifisch schwerere Silicate und Metalloxyde, vorzugsweise durch Augit und Magneteisenstein verdrangt wird." * * * "Wenn man so den Bau unserer Erdkruste betrachtet, gelangt man bald zu der entschiedenen Ansicht, dass alle diese crystallinischen Gesteine eine einzige continuirliche Kette bilden, deren Glieder innig mit einander verwebt mit den ältesten granitischen Formationen beginnen und mit den neuesten Laven endigen."

† Op. cit., p. 414:

"Um diese Mannichfaltigkeit von Erscheinungen zu erklären, greift Bunsen, das Einfachste, Naturgemässe, die im Allgemeinen gesetzmässige Disposition der Materie im Innern des Erdkörpers, zumal die zunahme der Dichtigkeit von der Erdoberfläche gegen die Tiefe hin, übersehend, zu einer sehr unwahrscheinlichen Hypothese von zwei gesonderten vulkanischen Herden, die getrennt von Einander saure und basische Silicate enthalten, durch deren Vermischung oder Verschmelzung jene unzähligen

In 1855 Lyell * discusses the supposed connection between the composition of igneous rocks and the age of their eruption, the most silicious being always considered the oldest, and remarks that "the hypothesis suggested by Mr. Scrope may perhaps afford a solution of this problem." But the explanation which he proceeds to give was not derived from the suggestions of Scrope already quoted, as will be seen by the following: "If, therefore," says Lyell, "a large quantity of rock be melted up in the bowels of the earth by volcanic heat, the denser ingredients of the boiling fluid may sink to the bottom, and the lighter, remaining above, would in that case be first propelled upward to the surface by the expansive power of gases. Those materials, therefore, which occupy the lowest place in the subterranean reservoir will always be emitted last and take the uppermost place on the exterior of the earth's crust." This he repeated in 1871.†

This is a separation by specific gravity, which was suggested by Darwin and modified by Dana, Scrope's theory being one of vaporization and squeezing out.

Two years later, in 1857, Jukes ‡ argues against the idea that there was a relation between the mineral composition of igneous rocks and their geological age—a view also combatted by Scrope, but advocated by von Waltershausen and Lyell.

Jukes would connect the composition with space or locality; thus he thinks "that the proper quantity of silica is referable to the depth at which an igneous rock has been

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Gesteinsnuancen hervorgegangen sind." * * * "Als eine blosse Fiction könnte man diese Hypothese wohl hinnehmen, nur muss man nicht glauben, dass sie in der Natur begründet sei, oder dass wirklich im Innern der Vulkane zwei gesonderte Herde mit basischen und sauren Flüssigkeiten vorhanden wären."

* Lyell (C.) A Manual of Elementary Geology. 8°. New York, 1855, p. 522.

† Lyell (C.) The Student's Elements of Geology. 8°. London, 1871, p. 503.

‡ Jukes (J. B.) The Student's Manual of Geology. 8°. Edinburgh, 1857, p. 81.

cooled or consolidated, or to the nature of those it penetrated." He remarks that the greater the depth the greater the heat, and that the more refractory minerals may be melted at the greater depths. The bases he considers as fluxes. He questions whether the more basic portions were present in the deepest seated molten masses, and were separated "on the first cooling and crystallization of the simple minerals," or were acquired during its progress to the surface. He suggests that one portion might consolidate at one depth, a more basic might reach a higher level, and so on; "so that from the very same stream of igneous matter proceeding from the interior to the surface of the earth, the more readily fusible portions might be successively squeezed out, as it were, as the infusible ones solidified and contracted in consequence of that solidification. This action might take place in spite of the greater specific gravity of the more fusible minerals, since the difference of the specific gravity would probably be small compared with the power of the eruptive force."

Jukes also suggests that in place of the terms "ancient" and "modern," as applied to igneous rocks, be substituted the expressions "deeply formed" and "superficially formed."

In accounting for the more basic varieties of rocks he says: "These more readily fusible substances might be conceived either to have separated in liquid strings or veins from the consolidating rocks below, or to have been acquired by the upper portion of the mass from the rocks it met with in its passage toward the surface, the substances thus added having acted as an additional flux to matter which would otherwise have solidified before it could have been poured out.

"Some such hypothesis as this seems to me less forced than one which obliges us to suppose separate deep-seated foci or reservoirs for every variety of igneous rock, those varieties frequently occurring in the same district and alternating one with the other over the same space of ground.

"If it be well founded, it will enable us to account for the gradual changes in one connected igneous mass, as also for the veins and patches of different character sometimes to be found occurring very abruptly in such masses, independently of the supposition of a subsequent intrusion of one igneous rock through the body of another. This would often relieve us of a difficulty where the veins are confined to the igneous rock and do not penetrate the adjacent aqueous rocks. We might then look upon such veins as veins of segregation, occurring probably at the time of the contraction consequent upon the mass of the rock passing from a molten to a solid state or from a pasty to a crystalline state while yet some parts of it remained fluid."

In this hypothesis there is the partial solidification of a molten magma at some depth and the squeezing up of the unconsolidated portion, together with the suggestion that the molten magma gathered up new material from the rocks through which it passed.

In the same year, 1857, Durocher published his "Essay on Comparative Petrology," * in which he enunciated the view, which he claims † was original with himself, that all rocks, modern and ancient, have been produced simply by two magmas which coexist beneath the solid crust of the earth and occupy a definite position.

These magmas have not changed in themselves during long ages, and differ from one another by distinct characters. One he calls the "magma acid" or "siliceux," and the other the "magma basique."

He observes that there are greater chemical differences between different kinds of granite than between granite and trachyte or pumice; therefore one may conclude that "for

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*Durocher (J.) Essai de Pétrologie Comparée, ou recherches surla composition chimique et mineralogique des roches ignées, sur les phénomènes de leur emission et sur leur classification. Ann. des Mines. 8°. Paris, 1857, vol. 11, pp. 217–259.

†Appendice au Mémoire intitulé Essai de Pétrologie Comparée, etc., p. 677, same volume.

rocks derived from the same magma the differences in the mineralogical characters depend less upon their elementary composition than on conditions of pressure, temperature, and, in general, on the circumstances of their cooling—that is to say, on conditions of an external character rather than on those of an internal character." *

The zone of contact between these two magmas produces intermediate rocks or "*roches hybrides.*"

The permanence of the separation of the two magmas is due to the difference in their specific gravities. There is, therefore, a fluid zone beneath the solid crust, the upper part of which is silicious, the lower basic. It would scarcely seem necessary for Durocher to consider this continuous body of molten magma, the middle of which is intermediate in character between the top and bottom portions, as two bodies of magma, except that he conceived the intermediate rocks to constitute a very inferior part of all igneous rocks. His conception more closely resembles Von Waltershausen's than Bunsen's; but he realizes that recent eruptive rocks are both silicious and basic, and correspond to ancient ones in composition, and consequently that his zone of magma must be the same now as in early geological ages, and capable of erupting silicious magmas as well as basic ones.

In order to explain certain minor variations in igneous rocks Durocher introduces the process of *liquation*, and says that "the magmas which have produced igneous rocks are comparable to baths containing several metals in a state of fusion, and which upon congealing separate into different alloys, according to the circumstances of their solidification,

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* Op. cit., p. 220:

"Pour les roches dérivant d'un même magma, les différences dans les caractères minéralogiques tiennent moins à leur composition élémentaire qu' à des conditions de pression, de temperature, et, en general, aux circonstances de leur refroidissement, c'est-à-dire à des conditions *d'un ordre externe* plutôt que *d'un ordre interne.*"

although the original bath presented the same composition."*

In the appendix already mentioned he refers more definitely to "the products of the phenomena of liquation which have operated within the liquid or viscous mass and have separated it into different compounds."†

This appears to have been the first comparison of rock magmas to alloys, and the conception of their separation into bodies of different composition by processes of liquation, though applied to only a limited range of phenomena.

Durocher also imagined that the access of sea water to molten magmas affects their chemical composition, rendering them more alkaline. He also argues that because of the independent action of neighboring volcanic vents, and of the irregularity and often weakness of their action, it does not seem likely that these vents connect directly with the general reservoir of magma, but only with secondary bodies in branching fissures ("foyers secondaires"). They are thus in a measure local.

In 1858 Von Cotta ‡ advanced the theory that the solid crust of the earth consisted of highly silicious substances, and that the fused portion beneath had about the composition of the most basic rocks, and that the variations in the composition of eruptive rocks were due to the variable amount of the solid silicious crust which was taken up by the basic magma during its passage toward the surface of the earth.

In 1861 Justus Roth § explained the various mineralogical

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* Op. cit., p. 220:

"Les magmas qui ont produit les roches ignées sont comparables à des bains contenant à l'état de fusion plusieux métaux, et qui, en se figeant, se partagent en des alliages divers, suivant les circonstances de leur solidification, lors même que le bain primitif offrait la même composition."

† Op. cit., p. 677:

"Les produits de phénomènes de liquation qui se sont opérés au sein de la masse liquide ou pâteuse, et l'ont partagée en des composés différentes."

‡ Von Cotta (B.) Geologische Fragen. 8°. Freiberg, 1858, p. 78.

§ Roth (J.) Die Gesteins-analysen in tabellarischer übersicht und mit kritischen Erläuterungen. 4°. Berlin, 1861, p. xix.

facies of one body of igneous rock by the separation (*spaltung*) of the original mass into different mineral associations; but he did not apply this principle to the development of certain varieties of rocks from some pre-existing magma, as Durocher did, nor did he consider the general question of the origin of igneous rocks. The same may be said of his more recent publications in 1869, 1887, and 1891, to which reference will be made in another connection.

In 1862 Jukes,* after referring to Bunsen's and Durocher's theories, remarks: "The identity or very great similarity of the various volcanic products in all parts of the world seems to point to a common origin for them. The frequent association in all parts of the earth of the two great classes of these products, the trachytic or purely feldspathic (or highly silicious, with little alkali, lime, or iron) and those in which the feldspathic are largely mingled with hornblendic or augitic minerals (containing much alkali, lime, and iron), seems to me to show that their separation is not so much due to diversity of origin as to some cause tending to segregate the one from the other out of a generally diffused mass in which the constituents of both may be equally mingled.

"If we assume all igneous rocks to proceed either from one central molten mass of equable constitution throughout or from separately fused portions of perfectly similar constitution, might we not suppose that the difference in the constitution of the various products which we find at the surface depended on the circumstances and conditions in which they had been placed? The portions now open to our examination had probably to pass through different conditions of temperature and pressure, which might perhaps alone cause a separation to take place in their different ingredients. They might also take up in their passage other ingredients, or more of some of those they already contained." He also suggests that at times when violent accessions of heat ap-

*Jukes (J. B.) The Student's Manual of Geology. 8°. Edinburgh, 1862, pp. 340–341.

proached the surface trachyte and felstones might be poured out; at other times more basic rocks.

In these expressions he refers all igneous rocks to one common magma, but does not suggest any but the vaguest causes for their differentiation.

In 1866 Zirkel* reviewed at considerable length the theories of Bunsen, Von Waltershausen, Von Cotta, Durocher, and the methods of classification of Kjerulf, Roth, and Scheerer, without advancing any particular hypothesis of his own with regard to the origin of igneous rocks. He appears to adopt that of Von Waltershausen as the most satisfactory.

In 1868 Von Richthofen, in his celebrated paper on "The Natural System of Volcanic Rocks,"† discusses at great length the origin of volcanic rocks. He attempts to establish a distinction between the products of volcanic action and massive eruptions, and states that "volcanic action and massive eruptions, notwithstanding the similarity of the material produced by both, would appear * * * to differ to some extent, not only in regard to the causes to which they owe their origin, but also in regard to the position the matter occupied before its ejection." He observes that the law of Bunsen is true for all eruptive rocks, but this has been abundantly disproven by Roth and others.

He considers that "the source of volcanic rocks has been similar in nature in every locality; that the definite numerical relations must exist at that source, and that at the same source the different kinds of matter which correspond to the different passages in composition among volcanic rocks must be arranged, at every locality alike, in a definite order of position vertically, in order to explain the order of succession of eruption." He observes that the arrangement of layers of increasing specific gravity within the earth was first stated by Von Waltershausen.

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*Zirkel (F.) Lehrbuch der Petrographie. 8°. Bonn., 1866, vol. 1, pp. 453–473.

†Von Richthofen (F.) The Natural System of Volcanic Rocks. 4°. San Francisco, 1868.

The application of his views as to the causes of ejection and the crystallization of rock at depths in consequence of the relief of pressure and of Von Waltershausen's law is that in the earliest periods the eruptive rocks must have been mostly silicious. The beginning of the volcanic period (the Tertiary) opens with basic rocks and is followed by still more basic ones.

As an exception to this rule he notices trachyte and rhyolite, but considers them as "a notable but only apparent anomaly," more closely related to andesites and only very remotely related to basalt. They are due to secondary processes, and are subordinate in regard to quantity.

The order in which the volcanic rocks were erupted is, first, *propylite*, then *andesite*, *trachyte*, *rhyolite*, and *basalt.*

He considers volcanic action to be closely related to massive eruption, but not so deeply seated, and remarks that two neighboring volcanoes may be erupting different kinds of lava and be independent of each other. Volcanic action is the last stage of massive eruption, but the order of succession is the same as with massive eruptions.

Von Richthofen advocates Von Waltershausen's theory, assumes that Bunsen's law is of universal application, and reiterates the old notion that the earliest eruptive rocks were the most highly silicious, and that they have been getting more and more basic to the present. His views of the action of volcanoes agree with those of Durocher, that they are derived from localized reservoirs that have branched off from the main supply.

In 1872 Scrope, in the second edition of his "Volcanos," advocates the theory that the separation of minerals of a molten lava within the focus of a volcano may be brought about by their differences in specific gravity, the feldspars rising and the augites sinking. He remarks that Darwin was of the same opinion, and refers to his book on the Volcanic Islands, which has already been cited. Scrope adds that "a somewhat similar effect may be occasioned by the mechanical squeezing out of the finer particles, or the more

readily fusible elementary minerals, from among the coarser or the less fusible, under extraordinary and locally varying pressures." He considers that the partial crystallization of augite and feldspar in subvolcanic magmas and the previous processes of separation probably account for the succession of flows of basalt, trachyte, and clinkstone alternating with one another.

In 1878 King * contributed a very important chapter to the question of the origin of volcanic rocks.

By the breadth of his treatment and by better and fuller data he advanced the problem of the origin of the various kinds of volcanic rocks far beyond the point reached by any of his predecessors, although at the same time he was led by the supposed establishment of certain criteria of distinction to separate the so-called Plutonic rocks, including granite, syenite, and diorite, from volcanic rocks; so that his hypothesis was confined to volcanic rocks and did not include all igneous or eruptive rocks.

After elaborating the order of succession of volcanic eruptions established by Von Richthofen, so that each member of the latter's order was subdivided into a hornblendic, a quartzose, and an augitic variety, which followed one another in the order given, he enters upon a consideration of volcanic fusion. After reviewing the principal theories with regard to the fused condition of the interior of the earth, he remarks that "the greatest single difficulty which the whole theory of fusion has to contend with is the extremely localized character of its phenomena, the fact of the non-sympathy of adjacent volcanic regions, and the chemical diversities of successive and contemporaneous products." And, having disposed of the hypothesis of a general molten interior, and of the residual lakes of Hopkins, and of the mechanical production of local lakes of fusion by Mallet, he asks, "What possible cause can there be to account for those extremely localized and only temporarily existing pools of fusion within

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* King (C.) U. S. Geol. Exploration of the Fortieth Parallel. 4°. Washington, 1878. Vol. I, Systematic Geology, p. 705 *et seq.*

the earth's superficial shell which the facts of volcanic geology demand?

The hypothesis which he advances to produce such conditions is based on the supposition that the rate of increase of pressure within the globe is greater than the rate of increase of temperature, and that the point of fusion is so far increased that the substance of the globe is in a state of rigidity. Upon a sufficient relief of pressure at any spot the fusion point of the material at that place becomes sufficiently lowered to allow of its being melted.

This diminution of pressure, he considers, may be brought about by the erosion of the superincumbent material at the surface of the earth. "So that the isolated lakes of fused matter which seem to be necessary to fulfill the known geological conditions may be the direct result of erosion."

In considering the "genesis of volcanic species" he reviews at length Von Waltershausen's theory of concentric molten shells of increasing specific gravity, which theory, he remarks, answered the conditions of the natural succession of igneous rocks, as far as Von Waltershausen's idea of that succession had progressed; but Von Richthofen, in adopting Von Waltershausen's theory, was met with the difficulty of accounting for the succession of eruptions he had himself established, since the highly silicious magmas were erupted between basic ones. He was therefore forced to imagine the source of the material to have risen from an intermediate position to a higher, and then to sink to a still lower one. This, King asserts, would have to take place four times during the eruption of the volcanic series since the commencement of Tertiary times; and when the igneous rocks of pre-Cambrian and Paleozoic age are considered, which also embrace a similar range of chemical variations, not only must the oscillation of the source of eruption have taken place during all these ages, but the graduated zone of molten matter must have remained unconsolidated for this length of time. Hence, he concludes, that "either this long continued oscillation from shell to shell, or, in view of the secular refrigeration, the per-

manence of these shells, or the eruption of pyroxenic material upward through the silicious shell, involves physical difficulties which appear to be altogether insuperable.

"If, however, by any means, temporary local lakes were formed, resulting from the fusion of a thin shell of the crust, it would seem that the arrangement into two zones—a lighter overlying a heavier one—would, from the nature of things, gradually assert itself within the limits of the enclosed fused region."

Assuming that his order of succession for each period of eruption is correct, namely, that the pyroxenic modification is always the later, the acidic being the first, he concludes "that wherever fusion is developed on a considerable scale, by whatever means, the fused material divides itself into two parts—the acidic or lighter coming to the surface before the basic and heavier.

"There are two methods by which the separation within the limits of a fused lake might be made: First, while in a state of fusion, on well-understood principles, the heavier liquid might concentrate at the bottom of the lake, leaving a supernatant couche of lighter matter; or, secondly, in the act of crystallization, which all present facts tend to prove is a subterraneous process, the actually formed crystals might separate themselves according to their differences of specific gravity;" an idea which he credits to Scrope.

King's theory is briefly as follows: Each one of Richthofen's orders, with its acidic and pyroxenic members, would be the product of a single ephemeral lake. A period of erosion would result in the formation of a lake. The cessation of erosion would define the time limits and the vertical expansion of the lake. Refrigeration, continuing from that time, would result in the crystallization of the various mineral species. There is sufficient difference in the specific gravity of the minerals of the acid and basic rocks to account for the separation. The magma through which they moved is partly the isotropic glass of the groundmass, partly the groundmass itself.

Since this separation would be an affair of some time and the causes which determined eruption might supervene when crystallization had begun and before specific gravity separation had completed its work, it would be natural to expect that eruption would frequently occur before the complete genesis of species. The latest lake of fusion after gravity separation would result in a layer of rhyolite floating upon a layer of basalt.

"Wherever a molten lake should be formed within the acidic shells of the earth, after separation by specific gravity, the relative proportions would show a great preponderance of the acidic member."

Refrigeration may advance so far before an eruption takes place that the upper layers of the lake may become solidified and only the lower portions be erupted.

Rocks of average composition may either result from the eruption of the fused lake before separation commences, or they may be the intermediate zone between the upper and lower layers.

In the secular refrigeration of the globe these temporary lakes of fusion would necessarily occur at greater and greater successive depths. The deepest of all would be the latest (neolite) lake, or that which produces rhyolite and basalt.

King recognizes the difficulty of explaining the presence of mica and hornblende among the lighter minerals of the acidic rocks, since their specific gravity should have carried them down into the lower portions of the lake. His explanation, however, is quite insufficient.

From the foregoing it would appear that the five kinds of volcanic rocks distinguished by Von Richthofen, namely, propylite, andesite, trachyte, rhyolite, and basalt, were each the product of a single ephemeral lake of fusion, except that the last two were derived from one source. Each would require for its production a special act of erosion, of cessation of erosion, and a crystallizing process before eruption.

In 1880 Dutton * advanced a theory for the eruption and succession of volcanic rocks which differs almost entirely from any of those heretofore mentioned. It starts on the assumption that the average composition of the foliated rocks on the surface of the earth is very nearly the same as that of basalt, with the exception of the iron oxide.

As to basalt, he says: "In truth, its whole range of characters is indicative of an origin among magmas which have never passed through the reactions and mechanical processes which prepared and arranged the materials of the sedimentary strata. Lastly, the basalts are among the most abundant of eruptive rocks, and, if we reckon with them the more ancient dolerites or diabases, they have always been abundant in all ages as far back as our knowledge extends; but not only should we infer that the primordial masses of the earth (or "primitive crust") were basic like the basalts or dolerites, but that they were very nearly homogeneous." The latter condition, he thinks, would have been brought about by the vast currents within the molten mass, which would have kept it thoroughly mixed.

"We know of no natural processes capable of separating the more acid parts of such a magma, except the chemistry of the atmosphere acting at temperatures far below the melting points of the silicates. We have the results of that process in the quartzites, granites, gneisses, and syenites among the silicious rocks, and the limestones and dolomites among the basic rocks, with argillaceous rocks as the residuum of the decomposition." * * * "No facts are known to me which justify the conclusion that segregation into two magmas could occur in such a state of fusion."

In commenting upon King's theory of crystallization and separation by differences of specific gravity, he considers it "extremely doubtful whether any crystals are formed while the rocks are melted, and very probable that the greater part of them are formed during the viscous stage of cooling."

*Dutton (C. E.) Geology of the High Plateaus of Utah. 4°. Washington, 1880, pp. 123 *et seq.*

One objection to the theory that a single magma has separated in this way into two or more of very different degrees of acidity is the fact that the low percentage of silica in basalt is not confined to the feldspar and augite, but is also in the base, while the high percentage in rhyolite is in the feldspar and still more in the base. Hence the segregation must have affected the base even more than the crystals.

He does not think that the acid or even the intermediate varieties can have been primordial masses and have remained liquid from the original earthmass to the time of eruption. The primordial magma ought to possess a constitution similar to basalt. "And, in general, our inference from the nature and constitution of the volcanic rocks, from their great variety, from the localization of eruptive phenomena, from the intermittent character of volcanic action, from the independence of the several vents, is that the lavas do not emanate from an earth nucleus wholly liquid, nor from great subterranean reservoirs still left in a liquid condition 'from the foundations of the world,' but from the secondary fusion of rocks, a part of which may have formed the primitive crust, while the remaining part consisted of deeply buried and metamorphosed sedimentary strata. We must at least admit that *the source of lavas is among segregated masses of heterogeneous materials.*"

With regard to the dynamical cause of volcanic eruptions he observes that "there is one general assumption which satisfies all the main requisites of volcanism. It is this: *Volcanic phenomena are brought about by a local increase of temperature within certain subterranean horizons.*" But this does not solve the problem, since it throws us back on the question, What produced the increase of temperature?—a question which he confesses he has been utterly unable to answer. There is an alternative assumption, namely, "Deeply seated rocks in regions of high temperature undergo changes, one result of which is to lower their melting point."

"A relief of pressure is one conceivable mode. Probably

another would be the absorption of water under great pressure and at high temperature."

He considers that King's theory that the relief of pressure has resulted from the erosion of superficial material is not in accordance with the geological conditions observed in many regions of volcanic activity. In many such regions there has not only been little or no erosion, but in some cases even there has been recent deposition.

"From a dynamical standpoint," he adds, "the problem to be explained is the passage of lava-forming materials from a dormant to an energetic condition," and that this means the passage of solid materials into the liquid condition with a decrease of density, and this involves, he thinks, an increase of temperature.

As to the mechanics of eruptions he remarks that "a careful examination of the details of volcanic eruptions leaves the impression that they are pressed up by the weight of rock which overlie their reservoirs, and that their extravasation is merely a hydrostatic problem of the simplest order."

"Lava will rise to the surface or not, according to its density. If it be lighter than the mean density of the rock above the reservoir, it will reach the surface; if it be heavier than the overlying rock, it will never reach the surface."

Upon this conclusion is based his explanation of the order of succession of volcanic rocks, which is the same sequence as that of Von Richthofen. The two essential conditions for their eruption are: First, they must be fused; second, they must be lighter than the overlying rocks. He considers the most basic rocks to be the most fusible, but that they require a higher temperature to be sufficiently light to reach the surface of the earth, while the acid rocks fuse at the highest temperatures, but are the lightest. From this he argues that the intermediate varieties may be fused and attain sufficient lightness first, and that the others follow in the order observed. He thinks that the basic rocks have reached the surface in a superfused condition, which gives

them greater fluidity. Since the source of the fused magmas is in heterogeneous material, and since the increase of temperature is an unexplainable condition, which may behave very erratically, it is to be expected that the sequence of eruptions will not always be constant, and apparent exceptions are to be looked for. In fact Dutton is surprised to find as much uniformity in the order of succession of volcanic rocks as is observed.

Since 1880 the science of petrology has been developing along new lines, and its followers have been busy establishing in great detail the exact mineralogical character of all crystalline rocks, as well as advancing their chemical investigation. A more thorough exploration of their geological occurrence and the finding of more favorable localities for the study of their field relationships have led to a clearer understanding of their true nature and have paved the way for an advance in the theory of their origin. Within recent years no comprehensive discussion of the nature and origin of igneous rocks has been published, but more or less fragmentary contributions to the subject have appeared.

In 1885 Teall * discusses the possibility of a partial crystallization and separation by gravity of an originally homogeneous magma in explanation of the varieties of rocks in the Cheviot District, but adds nothing new to the theories already based on this idea.

A new impulse was given to the science by the work of Lagorio † "On the nature of glass base, as well as the processes of crystallization in eruptive magmas," published in 1887. As the title indicates the paper is confined to a study of the glassy groundmass of rocks and to their crystallization. It treats the question from a chemical and physical stand-

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* Teall (J. J. H.) On some Quartz-felsites and Augite-granites from the Cheviot District. Geol. Mag. 8°. London, December, 1885, vol. 2, No. 3, pp. 106–121.

† Lagorio (A.) "Ueber die Natur der Glasbasis, sowie der Krystallisationsvogänge im eruptiven Magma." Min. u. petr. Mitth. 8°. Vienna, 1887, vol. 8, pp. 421–529.

point and brings into prominence those theories of magmas which consider them to be alloys or solutions of silicate molecules capable of becoming supersaturated with one or another of these compounds. It considers the effect of changes of temperature and pressure on the degree of saturation, and also the influence of absorbed gases on crystallization. He considers it most probable that the different silicates remain as such or as similar but constant compounds in the molten magma, by which is meant only the fundamental compounds ("Grundverbindungen"). He further assumes that the same minerals always crystallize from similar magmas.

It is to be remarked that however important these conceptions of the crystallization of igneous magmas have proved in their application to the broader questions affecting the origin and relationships of igneous rocks, Lagorio completely failed to appreciate it; for in the very paragraph in which he applies the principle discovered by Soret to the explanation of the separation of the material of certain dikes he makes use of Bunsen's hypothetical pyroxenic and trachytic magmas, and states in a foot-note that he considers Bunsen's hypothesis for the origin of different lavas as not having been done away with by any means.

Soret observed that if different parts of a solution of a salt be maintained at different temperatures there will be a concentration of salt in the colder part—a principle involving differentiation, the antithesis of Bunsen's synthetical theory.

The application of this principle to igneous magmas in general was subsequently suggested by Teall,* in 1888, in his work on "British Petrography." In discussing the crystallization of igneous rocks he quotes Lagorio's explanation of the differences between the center and walls of certain dikes, and adds, "Soret's principle may also be applicable to plutonic rocks. Homogeneous molten masses may become heterogeneous by diffusion of certain constituents during

* Teall (J. J. H.) British Petrography. 8°. London, 1888, p. 403.

cooling. Thus an intermediate magma may become differentiated into a basic and an acid magma."

In 1889 Rosenbusch,* in a paper on the chemical relations of eruptive rocks, adopts the theory of the differentiation of an originally uniform magma in order to explain the chemical differences of eruptive rocks. The subject is treated solely along the line of chemical investigation and is divided into three parts. The first deals with the question whether there are differences in the chemical composition of eruptive rocks of different geological periods, which is answered in the negative. The second, which constitutes the essential part of the paper, considers the character of the different phases of chemical constitution of the rocks. The third part consists of a single paragraph in answer to the question, How may the differences of eruptive masses at one and the same center of eruption be explained?

In his treatment of the second part Rosenbusch reviews Bunsen's theory and points out Roth's observation that the discordance between the theoretical and calculated analyses would be still greater than it is if some other constituent besides silica had been chosen as a basis of calculation. He compares with it Durocher's theory and emphasizes the latter's hypothesis of liquation as applied to molten magmas, by which Durocher implied that liquation is a spontaneous separation of a chemically homogeneous molten magma into several chemically different liquid magmas whose differences depend on the circumstances of their solidification. This Rosenbusch interprets to mean that similarly constituted whole magmas must not necessarily separate into the same partial magmas; and he states that it is one of the objects of his communication to show the untenableness of this assumption, and adds that the "ganggesteine" furnish the sharpest weapons with which to refute it, for their different varieties are connected in the most persistent manner with certain "tiefengesteine."

* Rosenbusch (H.) "Ueber die chemischen Beziehungen der Eruptivgesteine." Min. u. petr. Mitth. 8°. Vienna, 1889, vol. 11, pp. 144–178.

He further states that Durocher confused this idea of liquation with the separation of molten magmas upon solidification into different associations of minerals, according to circumstances, which view is definitely expressed by Roth in his "Tabellarischen Uebersicht der Gesteins analysen" in 1861. In this he states that molten magmas of the same or very nearly the same composition separate into different associations of minerals. A consequence of this would be, Rosenbusch remarks, that we should not be able to determine the mineral composition of a rock from its chemical analysis, to which no petrographer at this date would consent. With this conclusion the writer begs leave to differ.

Rosenbusch then defines his use of the term "spaltung," for which we will use *differentiation.* It is the spontaneous separation of a chemically homogeneous whole magma into two or more chemically different partial magmas.

In order to demonstrate the existence of such a differentiation he arranges a number of chemical analyses of granular and extrusive rocks in a table, which is finally reduced to a form in which it expresses the proportions of the metal atoms in each analysis. He then calls attention to the fact that the numerous different directions in which these rocks vary would necessitate as many different original magmas if the different rocks were the mixtures of extreme magmas.

He expresses the opinion that the original magma within the earth from which all eruptive rocks have been derived must be considered to have been originally homogeneous. This magma must have separated into partial magmas in order to produce the different eruptive rocks with which we are acquainted; and it is evident that this process has followed chemical affinities and has not been by chance.

The fact that certain magmas are of common occurrence while others are wanting must depend upon the principle that certain elements in molten solution mutually condition and exclude one another. This assumption necessitates the differentiation of the original magma. The occurrence as well as the regular recurrence of definite eruptive masses

removes the element of chance and furnishes occasion and basis for the search for the laws according to which differentiation has taken place.

In discussing the tables of atomic proportions he first selects five analyses in which the alkalies are unusually high and the bivalent elements are extremely low, and concludes that they represent the composition of a magma in which the relations of the alkali metals to aluminum and silicon are 1 : 1 : 2, there being other constituents present in small amount. This hypothetical magma he terms *foyaite* magma. Since this magma is known equally well in the form of "deep-seated," "dike," and extrusive rocks he concludes that it is not capable of further differentiation, or, more correctly, it is only slightly inclined to further differentiation.

From another group of analyses, which seem to have a rather wide range of composition, he derives an approximation to an atomic proportion of $(Na + K + 2\ Ca) : Al : Si$ of 1 : 1 : 2, and the magma represented by these analyses is considered to have this molecular constitution with an excess of silicon, and is called the *granitic* magma.

It is assumed that a magma that is capable of separation will be more highly differentiated the more advanced its geological formation, which is explained as meaning that extrusive rocks as a whole are more highly differentiated than deeply seated ones.

Another group of analyses furnishes the basis for a *granito-dioritic* magma, in which the following relation obtains: $Na + K > Ca < 4\ Ca$, and in which there are considerable amounts of Mg and Fe. This comprehensive magma produces granitite, amphibole- and augite-granite and their modifications, together with syenites and dioritic rocks with their extrusive equivalents. Its characters are therefore vague. It is considered to consist of a "metal kern," $(Na\ K)\ Al\ Si_2$, with another, $Ca\ Al_2\ Si_4$, besides which Al and Ca occur at times in excess. The large proportions of Mg and Fe in this magma is accounted for by the sup-

position that they are capable of dissolving larger amounts of non-aluminous magnesian and iron compounds than the two magmas first named. This magma is considered to be specially susceptible to differentiation, leading to the production of aplite and the lamprophyric dike-rocks. It seems remarkable that magnesia and iron should not have been considered essential ingredients of the hypothetical "metal kerns" of this magma, since they are such essential ingredients of the rocks from which the magma has been deduced.

Another group of analyses, representing basic diorites, gabbros, porphyrites, melaphyres, diabases, andesites, and basalts, differs from the previous one by a greater proportion of the "metal kern," $Ca\ Al_2\ Si_4$, and a smaller proportion of the "metal kern," $(Na\ K)\ Al\ Si_2$, with more Mg and Fe. These magmas are called *gabbro* magmas, and are considered to be very much mixed.

The last group of analyses includes those with large amounts of Mg, Fe, and Ca, and the hypothetical magma is called the *peridotite* magma. It is composed of the "metal kerns" $(Na\ K)\ Al\ Si_2$, $Ca\ Al_2\ Si_4$, $R\ Si$, and $R_2\ Si$ in various proportions.

A small number of analyses remain that do not fit into any of the preceding groups. The discussion of these analyses leads Rosenbusch to the conclusion that the aluminum in rocks is confined to a feldspathic kern, although one of the rocks under discussion is an augitite, with 24 per cent. of alumina, the mineralogical characteristic of the rock being the almost complete absence of feldspar. By these considerations Rosenbusch has been led to the conclusion that the magmas furnishing eruptive rocks arise from the differentiation of a primitive magma, and that by this process the *foyaite* and *peridotite* magmas contain the "metal kerns," $(Na\ K)\ Al\ Si_2$ and $R_2\ Si$ or $R\ Si$, in almost perfect purity; and, further, that these alloys or compounds are insoluble in one another, but that the "metal kern," $(Na\ K)\ Al\ Si_2$, has the power of dissolving Si and $Ca\ Al_2\ Si_4$. Finally, in

magmas with the "metal kern," Ca Al_2 Si_4, as the amount of this "kern" increases the magmas become more capable of dissolving the non-aluminous "metal kern," (Mg, Fe) Si.

The differentiation of the original magma furnishes the magmas which have consolidated into deeply seated rocks—that is, granular ones. The further or secondary differentiation of these magmas have furnished the corresponding extrusive rocks.

He remarks that if we should find the deeply seated rocks and the connected series of extrusive rocks of one region, a comparison of the chemical composition of both would establish the laws of the differentiation of magmas; and he adds that there can be no doubt as to the geological and genetic relationship between certain deeply seated (or granular) rocks and their accompanying system of dikes, and that here, then, is to be applied the test for the laws of differentiation, which in the paper just reviewed have been evolved from the chemical constitution of other forms of eruptive rocks. This is said to be the reason why the special forms of dike rocks which he has called "ganggesteine" have not been taken into consideration in this first communication on the subject.

Rosenbusch's paper has been critically reviewed by Justus Roth * in an article on the classification and chemical character of eruptive rocks. In it he states that he had used the term "spaltung" originally to cover what are known as facies of rocks, and cites instances in support of his statement that molten magmas of the same or similar chemical composition may separate into different associations of minerals. He remarks that he does not find that the analyses presented by Rosenbusch substantiate the law laid down by him, that the deeply seated rocks are richer in bivalent metals and poorer in alkalies and silica than the extrusive rocks.

In reviewing the treatment of the chemical analyses

* Roth (J.) "Die Eintheilung und die chemische Beschaffenheit der Eruptivgesteine." Zeitschr. d. D. geol. Gesell. 8°. Berlin, 1891, vol. 43, Part I, pp. 1–42.

selected by Rosenbusch he first points out a number which he thinks are of too greatly decomposed rocks to be serviceable. He then shows that the sum of the metal atomic proportions is in no sense characteristic of any group of rocks. He follows the calculations of the ratios which Rosenbusch made out for the different rock magmas and shows that they are too widely divergent to establish the hypothetical "kerns" derived from them. He calls attention to the number of varieties of rocks embraced within each of these "kerns," trachyte appearing in three, and shows the difficulty of deriving the minerals of rocks from the hypothetical "kerns."

In 1890, almost simultaneously with the article by Rosenbusch on the chemical relations of eruptive rocks, appeared a monograph by Brögger * on the minerals of the syenite-pegmatite dikes of southern Norway. As an introduction of this great work there is given an account of the geology of the region of Christiania with special reference to its eruptive rocks. This is but a preliminary sketch of what is eventually to be a monograph on these rocks—a book whose appearance will be looked for with the keenest interest. The igneous rocks of this region constitute a series of eruptions mostly of Devonian age, and form massive intrusions, dikes, and surface flows. There is a wide range of mineralogical and chemical composition and a well-defined order of eruption. The result of the investigation of these rocks has led Brögger to certain definite conclusions regarding their origin.

The whole group of eruptions forms a continuous series of rocks which merge into one another petrographically and chemically. Commencing with very basic rocks, they become more and more silicious, until the last of the series, which are basic. The variation of the chemical constituents other than silica is different in different cases. These relationships obtain for the coarsely granular rocks (Tiefenge-

* Brögger (W. C.) "Die Mineralien der Syenitpegmatitgänge der Südnorwegischen augit- und nephelinsyenite." Zeitschr. fur Kryst u. Min., 8°. Leipzig, 1890, vol. 16.

steine), the extrusive rocks, and to a great extent for the dike rocks.

The correspondence between the petrographical and geological sequences appears to Brögger to prove undoubtedly a genetic connection between successive eruptions; and this, he thinks, is substantiated by the fact that the greater part of the rocks occurring in this region are not found in any other part of the world, and that many of those which do occur elsewhere have an entirely different habit in the region of Christiania. Moreover, there are many rocks of common occurrence in other regions which are not found in the one just mentioned. The greater part of the series is characterized by a high percentage of soda. These relationships indicate that the successive eruptions must have originated from a common, separate, and enclosed reservoir of magma ("Magma basin"), which cannot be simply a portion of a universal fluid magma forming the interior of the earth.

He adopts Soret's principle as the probable cause of the differentiation of the magma, which is considered to be a solution of silicates, and suggests that possibly a partial crystallization may have set in at the same time, producing segregations of the more basic minerals.

A diffusion of the less soluble constituents having taken place toward the cooler margins of the reservoir, the first eruptions will be the most basic, and will be followed by more and more silicious ones. The final eruption of basic magma is possibly due to the accumulation at the bottom of the reservoir of crystals of basic minerals that have crystallized and settled, and have been melted again. The character of the whole series of rocks will depend on the composition of the original magma, which in this region, as already remarked, was rather rich in soda. The origin of the reservoir of molten magma is not discussed.

In the following year, 1891, Vogt * published a paper on

* Vogt (J. H. L.) "Om Dannelsen af de vigtigste in Norge og Sverige representerede grupper af jernmalmforekomster. Geol. Fören. i Stockholm Förhand." 8°. Stockholm, 1891, May, Vol. 13, p. 476. Reviewed by J. J. H. Teall in the Geological Magazine, Feb. 1892.

the formation of iron ore deposits in Norway and Sweden, a part of which he refers to "magmatic concentration" in strongly basic eruptive rocks. The bearing of this principle on the question of the variation of igneous rocks is pointed out by Teall, to whom we are indebted for a review of this important work from which the present notice is taken.

After discussing the mineralogical character of the iron ores in question and that of the rocks with which they are associated, he concludes that they have resulted from a concentration in the plutonic magmas for the following reasons:

(1.) The different ores stand petrographically related to the surrounding rocks.

(2.) The ore in many cases passes gradually into the surrounding rock.

(3.) In no case is there any evidence of the introduction of material by solutions. The segregations are characterized by the minerals of the surrounding rocks, and by these alone.

As to the process by which this concentration has taken place, he is of the opinion that it was by molecular diffusion without the actual separation of minerals.

"The homogeneity of a solution may be destroyed by temperature differences and by gravity. The influence of the former has been experimentally established by Soret, and follows as a necessary consequence of van't Hoff's theorem that osmotic pressure in the case of dilute solutions obeys the laws of gaseous pressure. The influence of the latter has been deduced experimentally by Gouy and Chaperon from the laws of thermo-dynamics. Where solutions become heavier by concentration the lower part will be more concentrated than the upper part. The difference is slight and can only be recognized with difficulty when a tube 100 meters high is used. The specific gravity of a molten magma will increase with an increase in the number of molecules of magnetite, ilmenite, magnesia, iron-silicates, pyrite, etc. Hence the molecules will, according

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to the law of Gouy and Chaperon, be more abundant in the lower than in the upper portion of a magma basin."

"Differences of temperature operating according to Soret's principle will cause the same molecules to accumulate in the colder portions of the same magma basin."

Another possible cause of concentration considered by Vogt is magnetic attraction, which may act after concentration has been started by some other force.

In a paper by the present writer on the eruptive rocks of Electric peak and Sepulchre mountain, which was prepared for the Annual Report of the United States Geological Survey for the year ending with June, 1891,* the opinion is expressed that "the chemical differences of igneous rocks are the result of a chemical differentiation of a general magma, and in a very special manner all of the igneous rocks of any locality are so intimately related to one another chemically that there is far more reason for considering them as a complex chemical unit than as a number of independent well defined magmas."

In the Report on the Geology of the Eureka District, Nevada, by Arnold Hague, now in press and about to appear as a monograph of the United States Geological Survey, in chapter VIII, on the volcanic rocks, Hague reaches the conclusion that all the volcanic rocks of that district have been derived from one general magma by a process of differentiation.

In a paper "On the Plutonic Rocks of Garabal Hill and Meall Braec," by J. R. Dakyns and J. J. H. Teall, which has just been published † and which reached the writer after the present paper had been read before the Society, May 7th, the investigators describe a series of granular rocks that form a connected group of intrusions ranging from ultra basic to

* Iddings (J. P.) The Eruptive Rocks of Electric Peak and Sepulchre Mountain, Yellowstone National Park. Twelfth Annual Report of the U. S. Geol. Surv. 8°. Washington, 1892, pp. 569–664.

† Dakyns (J. R.) and Teall (J. J. H.) Quart. Journ. Geol. Soc. 8°. London, 1892, May 2, vol. 48, part 2, No. 190, pp. 104–120.

highly silicious varieties. The rocks grade into one another, petrographically and chemically, and in some places in the field form continuous bodies of variable composition; in others they constitute definite bodies of different characters.

"Taking all the facts into consideration," the writers remark, "there seems no escape from the conclusion that we have in this area the record of a series of events connected with the consolidation of a vast subterranean reservoir of molten rock. Having regard to the whole mass, it seems probable that the process was a continuous one; but if we consider only certain limited portions, it was unquestionably discontinuous." * * * "The order of succession of the rocks is from basic to the most silicious. The order in which the essential minerals of these rocks commenced to crystallize from the magmas is said to be the same in all cases and to begin with the most basic and specifically heaviest and to end with the most silicious and specifically lightest. The variation in chemical composition is also considered and plotted in a diagram representing the molecular proportions.

In suggesting a cause for the observed relationship between the rocks of this group the idea is expressed that "the area represents a vast subterranean reservoir, which has become differentiated during the process of consolidation," and that the separation was a consequence of the formation of the minerals in the order just given.

"Other causes, such as Soret's principle, or the subsidence of the first formed crystals by gravity, may also operate and assist in producing heterogeneity in the original magma."

In attempting to apply this hypothesis to the problem of volcanic rocks it is concluded that the basic rocks should precede the acid rocks; but it is noted that such a rule is by no means without exceptions. "The exceptions may be due to the existence of two or more reservoirs, or to the accession of heat or fresh material during the process of consolidation."

It is also suggested that "the consolidation of a plutonic magma may give rise to rocks of which there are no volcanic

equivalents. Such rocks may result from the separation of minerals from a magma having a very different composition from that of the rock in question. The absence of volcanic representatives of the typical peridotites and their rarity as dikes may be due to this."

The paper closes with a notice of Vogt's hypothesis regarding the deposits of iron ore, which has been previously mentioned.

In the same number of the Quarterly Journal of the Geological Society that contains the article just noticed is the anniversary address of the president of the society, Sir Archibald Geikie. The address, together with that delivered at the previous anniversary, constitutes one of the most important contributions to the history of volcanic action, although it is confined to the area of the British Isles.

Without entering into a discussion of the origin of the volcanic rocks of the region, Geikie calls attention to the nature and composition of the lavas and to the sequence of their eruption, and makes it evident that their source is to be sought in changes taking place in the general magma from which they have originated.

Since many of the facts brought out in this comprehensive, but necessarily incomplete history have a direct bearing on the paper prepared by the present writer, allusions to them have been inserted in the body of the paper.

Having reviewed in considerable detail the most important contributions to the development of a theory of the origin of volcanic rocks, we may proceed to the statement of the geological, petrographical, and chemical data which appear to the writer to maintain the thesis of this paper. But before doing so the writer wishes to express his indebtedness to Mr. Arnold Hague, geologist in charge of the Yellowstone Park Division of the U. S. Geological Survey, for the freedom with which he has been permitted to use any information or material considered essential for the purpose just mentioned. This embraces geological observations as well as numerous chemical analyses, which have been prepared for the report on the Yellowstone National Park.

CONSANGUINITY OF IGNEOUS ROCKS.

The repeated association of different kinds of volcanic rocks in various parts of the world and in different volcanic districts led Scrope, Darwin, Dana, and others to the belief that they were derived from a common source by some process of separation.

On the other hand, the great variability of the rocks among themselves in particular regions led others to emphasize the points of difference, and to believe that the differences must have existed always, or have been the result of the mingling of widely different materials.

The first idea was based on broad geological observations; the second was inspired chiefly by chemical and physical considerations. Each idea involved the other to a greater or less extent, and approached nearer to completeness in proportion to the extent to which all of these conditions were taken into account.

At the time of the early speculations regarding the relationship and origin of igneous rocks little was known of the actual mineral composition and crystalline structure of rocks beyond what could be gathered from the crystals large enough to study with a pocket lens, which, in the case of a great proportion of them, is but a part of the whole rock. Even the chemical composition of the rocks was imperfectly known, owing to the incompleteness and insufficiency of the chemical methods then in use.

With the introduction and development of microscopical investigation the mineralogical character and the crystalline structure of igneous rocks have become better known. The first natural result of such detailed study has been to magnify the mineralogical differences of rocks, and to construct a complex classification based often on very slight differences, which, however, are more apparent as the rocks studied are fewer in number and are from different regions.

In studying all of the igneous rocks of any one region, the investigator is at first impressed with the differences in

the mineral composition of the various bodies of rock, even when the more thorough study brings to light the great number of intermediate varieties. In fact, when large areas of eruptive rocks are carefully investigated, it is found that there is a perfect and gradual transition of one kind into another—all intermediate varieties existing—and that quantitatively no special part of the series is universally predominant, although there are often immense masses of nearly uniform character, and there may be smaller bodies of quite variable composition. Nevertheless, the effort is made in describing such a group of rocks to select the rocks that differ most and make them serve as types to which specific names can be applied, alluding to the others incidentally as intermediate forms. The convenience as well as the error of such a method are apparent.

When, however, the rocks of many regions are studied, it is found that, notwithstanding the variations in mineral composition in the rocks of one district, there exist characteristic qualities of the minerals which are persistent for a greater or less range of rock varieties in that district, and which distinguish the whole group of rocks from those of some other district. The qualities or peculiarites are like family traits of character, and at once suggest the intimate relationship and common origin of all of the igneous rocks of the district. The same idea has been clearly expressed by Judd,* who states that "there are distinct *petrographical provinces* within which the rocks erupted during any particular geological period present certain well-marked peculiarities in mineralogical composition and microscopical structure, serving at once to distinguish them from the rocks belonging to the same general group, which were simultaneously erupted in other petrographical provinces."

It is, of course, not to be inferred from this that every district of eruptive rocks has some peculiar characteristic which

*Judd (J. W.) "On the Gabbros, Dolerites, and Basalts of Tertiary age in Scotland and Ireland." Quart. Journ. Geol. Soc. 8°. London, 1886, vol. 42, p. 54.

distinguishes it from every other district. On the contrary, there are many that appear to be identical with one another and undoubtedly are the same; but there occur in various parts of the world family groups of rocks which, while identical with other groups, are different from many surrounding them and constitute associations of rock groups. The recurrence of these associations establishes a still higher order of relationship or the existence of a more remote common origin.

It is to express the idea that all the igneous rocks of any volcanic district have been derived from a common stock—that is, from a common magma—that the writer has applied the term *consanguinity* to the relationship.

MINERALOGICAL EVIDENCE OF CONSANGUINITY.

To illustrate what are some of the characteristic qualities of the minerals and rocks of groups which are thus allied may be mentioned a few of the more easily recognizable features.

They are partly inherent in the minerals and partly consist in their association in the rock, affecting its habit. To a very great extent they depend on the chemical constitution of the magma of the rock, though it is not always possible to point out the connection, on account of the incompleteness of our knowledge of the chemical composition of the minerals in every case, and also because there are other conditions besides chemical ones which have influenced crystallization.

One of the most universal and persistent minerals in volcanic rocks is *augite*. It is one also in which the qualities referred to are most noticeable, since they affect its color. In a great many groups of rocks, and through a vast extent of territory in western America, the color of the augites in thin section is pale green, with no recognizable pleochroism or difference of color. Rarely it assumes a purplish tone in some ophitic basalts; but as the eastern flank of the Rocky

mountains is approached the augites assume a stronger green, and many varieties of rock contain grass-green augites and many others true ægerine and acmite, with their characteristic green and brown colors and peculiar crystal forms. In other districts, better known in Europe, groups of rocks are distinguished by purplish augites.

The relation which was pointed out by Judd * in 1876 as existing between the rocks of Hungary and those of Bohemia also exists between the volcanic lavas of the Great Basin of western America and the belt of country east of the Rocky mountains, including the vicinity of the Highwood mountains and the Crazy mountains in Montana; the Black hills in South Dakota, and Leucite hills in Wyoming and certain parts of Colorado.

In the region just mentioned are found phonolites, leucite rocks, and trachyte, with allied rocks, such as occur in Bohemia, Germany, France, Italy, the northwest portion of Africa, and the islands off its western coast. The rocks of the alkali belt in America have not been thoroughly exploited, so that our knowledge of them is very imperfect. We are better acquainted with those of the Great Basin and Pacific coast volcanoes, which are identical with the rocks of Hungary.

The variation of the pyroxene has been shown by Merian † to depend on the chemical constitution of the rock. The increase in the green color indicates the greater proportion of the acmite molecule, which carries the soda. The purplish augites are found in the rocks very rich in ferromagnesian oxides and titanic acid and characterize these groups, being most noticeable in some nephelinites. It is not to be understood that every rock in the region in which these varieties of augite occur is characterized by a particular kind of augite. On the contrary, there are districts where all of them appear

* Judd (J. W.) On the Ancient Volcano of the District of Schemnitz, Hungary. Quart. Jour. Geol. Soc. 8° London, 1876, vol. 32, p. 303.

† Merian (A.) Studien an gesteinsbildenden Pyroxenen. Neues Jahrb. Min., etc. 8°. Stuttgart, 1884, Sup. vol. 3, pp. 252–315.

to have been developed, two varieties sometimes occurring in one rock. Nevertheless there are localities where one or the other is almost the universal occurrence. In groups of rocks with pale green augite it will be found on close investigation that the tone of green is different in different districts.

Within narrower limits peculiar qualities of minerals distinguish parts of a series of rocks at one center of eruption. Thus the hornblendes of the intrusive porphyrites and andesites at Electric peak and Sepulchre mountain possess a grayish tone, which varies in intensity with the rocks, but is quite characteristic. The color of hornblende, however, within a large series of igneous rocks in one district is most variable, ranging from green in the granular rocks to greenish brown and reddish brown in the basic lavas.

The color of mica when fresh is often quite distinctive of different groups of rocks. The particular tones of color of minerals in such groups of rocks is more easily recognized by the eye than described.

The association of minerals in different localities is even more characteristic. Some of the more noticeable of these may be mentioned.

In many groups of volcanic rocks, including those of the Great Basin, the Pacific coast of North America, Central America, parts of the Andes, Hungary, and in Great Britain, *hypersthene* is an essential constituent with a very wide range, extending in many cases from certain forms of basalt through the andesites and dacites to many forms of rhyolite. It accompanies augite in nearly every instance and may be said to be almost universally present when the rocks are fresh, though more abundant in some cases than others. It continues to accompany augite in the granular equivalents of these rocks to a certain extent and is found in the porphyrites, diorites, and gabbros of the Yellowstone Park region. On the other hand, there are groups of basalts, andesites, and dacites in which it is as noticeably absent. These rocks are associated with trachytes, phonolites, and allied rocks. Thus it is not

found in a large part of the rocks of the Auvergne, the Siebengebirge, or Bohemia. It is equally scarce in the corresponding rocks of the alkali belt east of the Rocky mountains.

Biotite characterizes certain groups of volcanic rocks by its different range. There are many groups, including basalt, andesite, dacite, and rhyolite, in which biotite occurs in the more silicious andesites and continues through dacite into rhyolite. This is true of the rocks of the Eureka district and in many other localities in western America, but in the region of the Yellowstone Park the rhyolite and its dacitic facies are free from biotite—the range of the biotite being smaller and limited to silicious andesites.

In Arizona biotite characterizes more basic andesites and constitutes biotite-pyroxene-andesites grading into basalt. Biotite-pyroxene-andesites also occur in the Troad in Asia Minor and elsewhere. Biotite is an essential constituent of basic rocks under special conditions, as demonstrated in the case of intrusive rocks at Electric peak and of the very similar rocks in Crandall basin.

Orthoclase feldspar as an essential constituent of rocks low in silica—that is, trachyte in the strict sense—is characteristic of certain groups of rocks and particular localities. It does not occur in the groups found in the Great Basin and along the Pacific coast, but begins to appear as the belt east of the Rocky mountains is approached, and it is limited to certain belts of country in other parts of the world, to which attention has been called by Judd in the paper already cited. The development of phenocrysts of orthoclase in the less silicious forms of certain groups of rocks is a special phase of crystallization found in regions where trachytes occur, and is particularly characteristic of the belt along the eastern flank of the Rocky mountains.

The prevalence of sphene in particular groups of rocks and its almost total absence from others may also be cited.

Sporadic grains of primary *quartz* are a most distinctive feature of certain groups of rocks in special localities. It

characterizes the rocks of the Tewan mountains in New Mexico,* where it is found in variable amounts in basalt, pyroxene-andesite, hornblende-pyroxene-andesite, hornblende-mica-andesite, and rhyolite, the quartzes being colorless. It also occurs throughout a group of rocks in the vicinity of Crescent peak, Colorado, where the rocks range from basalt through andesite to what is possibly trachyte. In the Eureka district, Nevada, the whole group of rocks from basalt to rhyolite, including pyroxene-andesite, hornblende-mica-andesite, and dacite, is characterized by quite sporadic quartzes, which are found in all of the rocks though not in all of the hand specimens. In most all of these cases it is dark-colored; and dark-colored quartz is particularly characteristic of the rhyolite of this district, though in some forms of the rhyolite and of the other rocks it is colorless. Thus it is not only the presence of quartz grains in all the members of the rock group in each of these instances, but the particular quality of the quartz that distinguishes them.

The illustrations that have been briefly given are some of the more striking features of different groups of volcanic rocks. They could be elaborated until all of the mineralogical and structural characteristics of different rock groups were depicted; for it is the combination of particular minerals with their special crystal forms and modes of development which produces what we term the habit of a rock, and when large groups of rocks from different regions are compared it is often noticed that the habit of the rocks of one group is distinctive. But this is not the place to enter into such details, nor does the writer consider himself prepared to describe such features at length without further study. The investigation of the characteristic features of various rock groups is possible only when full collections have been made from the localities in question and when they can be compared directly with those of other regions.

* Iddings (J. P.) On a group of volcanic rocks from the Tewan mountains, New Mexico, and on the occurrence of primary quartz in certain basalts. Bull. No. 66, U. S. Geol. Surv. 8°. Washington, 1890.

CHEMICAL EVIDENCE OF CONSANGUINITY.

The consanguinity of the igneous rocks of one district is also proven by their chemical characteristics. Here also the same line of argument applies as that followed in discussing their mineralogical characters.

A study of the rocks of one group, like that of Electric peak and Sepulchre mountain * or that of Crandall basin,† convinces one of the gradual transition in chemical composition throughout the series of rocks, as well as of the variability of the molecular proportions. The chemical individuality of the group of eruptive rocks in the region of Christiania has been pointed out by Brögger in the work already cited in the historical part of this paper.

At Electric peak and Sepulchre mountain, as already described in another place, there is a series of rocks which pass by gradual transitions from basic diorite to quartz-mica-diorite and granite, and by gradations of crystallization into porphyrites of variable composition; and, finally, into andesites, basalt, and dacite. In the region of Crandall basin the series of rocks ranges from gabbro to diorite, quartz-mica-diorite, and aplite, and into porphyrites corresponding to these, and also into basalts, andesites, and unusual rocks of the lamprophyric kinds, with their feldspathic complements, which also occur in a somewhat different form as lava flows of alkali-feldspar-basalts, leucite-basalts, and trachytic tuffs. The petrography of these rocks will be found in a future publication of the U. S. Geological Survey.

The granular rocks of Crandall basin and the volcanic rocks associated with them form a natural geological group. The unusual lamprophyric rocks and the corresponding surface flows are partly connected with this center of eruption

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*Iddings (J. P.) The eruptive rocks of Electric peak and Sepulchre mountain, Yellowstone National Park. Twelfth Annual Report U. S. Geol. Surv. 8°. Washington, 1892, pp. 569–664; also Bull. Phil. Soc. Washington. 8°. Washington, vol. 11, pp. 191–220.

† Iddings (J. P.) The dissected volcano of Crandall basin, to appear as a publication of the U. S. Geol. Surv.

and partly with other neighboring centers where similar series of rocks have been erupted. For purposes of discussion they have been grouped by themselves as "Exceptional dike rocks, and lava flows in the Absaroka range."

Perhaps the chemical relationship between different varieties of rocks is at first more apparent than the mineralogical relationship, because they all contain the same chemical elements in only slightly different proportions, whereas the minerals are often quite different. When the chemical variability is considered in connection with the occurrence of the rocks in mass and it is realized how considerable a range of variation may occur in one body of rock, and when it is remembered that a number of bodies of rock may constitute overlapping series of variations, an original connection of the whole group of rocks becomes apparent.

The chemical composition of various rocks of the Electric peak and Sepulchre mountain group is shown by the analyses in Table I, to which have been added six analyses of rhyolitic lavas and obsidian from the Yellowstone Park plateau, which will be referred to subsequently. The former group range from 55.83 to 69.24 per cent. of silica and have been discussed in detail in the article on this district in the Twelfth Annual Report of the U. S. Geological Survey. A list of the rocks analyzed is given in connection with the tables.

The molecular variation of the principal oxides in this group of analyses is expressed by diagram 1, Plate 2, those of silica being taken as abscissas and the others as ordinates. The lines are drawn to connect the successive ordinates of each oxide in the group and indicate the direction of the variation. Diagram 1*a* shows the alumina and the alkalies by themselves, and diagram 1*b* the lime and magnesia, with the iron reckoned as ferrous oxide. The vertical and horizontal scales are the same in all cases and the lateral space allowed each diagram represents the same range of silica molecules—that is, from .765 at the left-hand end to 1.265 at the right-hand end, which corresponds to a range of from

45.90 to 75.90 per cent. of silica. The location of the diagrams laterally in each space indicates whether they belong in the more silicious or less silicious part of the series, and is a means of comparing their silica proportions with one another. All of the diagrams have been placed in juxtaposition in order that they may be compared more readily.

The chemical character of the rocks of the Crandall volcano is shown by the analyses in Table II. Their molecular variation is represented by diagram 2; the alumina and alkalies by 2*a*, and the lime, magnesia, and ferrous oxide by 2*b*.

The analyses of the exceptional dike and flow rocks of the Absaroka range are given in Table III. The molecular variation is shown in diagrams 3, 3*a*, and 3*b*. The noticeably high percentage of water in the analyses of this group is one of its characteristics. It is considerably greater in the less silicious rocks than in the more silicious. It is largely due to the presence of zeolites, probably analcite, which are recognized in some of the less silicious rocks. They appear to result from the alteration of a feldspathic constituent of the groundmass in some cases, the other constituents being unaltered, except the olivine, which is more or less serpentinized. However, it is not certain that all of the water is the result of decomposition, for one of the unaltered basaltic glasses (analysis 6, Table II), contains 2.99 per cent., and certain allied rocks, described by Lindgren,* from the Highwood mountains, Montana, contain abundant hydrous minerals, which appear to be of primary origin.

The analyses of several other groups of rocks from well-known European regions were selected to compare with these. They were taken from the tables of analyses published by Roth†. In doing this care was taken to use only those which had been made in recent years and bore evidence of greater

*Lindgren (W.) Eruptive rocks from Montana. Proc. Cal. Acad. Sci. 8°. San Francisco, 1890, Ser. 2, vol. 3, pp. 39–57.

†Roth (J.) Beiträge zur Petrographie der plutonischen Gesteine. 4°. Berlin, 1873, 1879, 1884.

completeness than can be found in older analyses. It is to be regretted that there do not exist more extensive sets of complete and accurate analyses of the rocks of special localities, since they are necessary for the proper comprehension of the nature of igneous rocks as a whole.

A series of analyses representing the different varieties of rock at Vesuvius was chosen from those made by Houghton and Ricciardi. To these were added analyses of the rocks of Ischia by Fuchs, and a facies of the Vesuvian magma occurring as a bomb, and two rocks from Rocca Monfina. They are given in Table IV, and the molecular variations are shown in diagrams 4, 4*a*, and 4*b*.

Another group of analyses shows the composition of the lavas of Etna, Table V, with two basic rocks from Pantelleria, and one from the Island of Ferdinandia, and another group gives the composition of the more silicious rocks of Pantelleria, Table VI. The molecular variations of these two groups are expressed in diagrams 5 and 6, and by 5*a*, 5*b*, 6*a*, 6*b*. The two groups of rocks are geographically close to one another and belong to the same general region. For present purposes it is not necessary to consider the mineralogical character assumed by these magmas. A list of the rocks from which the analyses have been made will be found in connection with the analyses.

The chemical individuality of any group is best appreciated when it is compared with the chemical character of some other, for with so wide a range of variability it is not always evident where the characteristic quality is to be found, and yet it is sometimes observed upon comparing groups as closely situated as those already mentioned in the Yellowstone Park region, for we are passing from the Great Basin type into the Eastern zone. The characteristic difference between these two groups lies in the alkalies. In that at Electric peak and Sepulchre mountain the ratio between the soda and potash molecules is from 3:1 to 2:1, their variation from the less silicious to the more silicious end of the series being more gradual and less irregular than that of the other elements.

In the group at Crandall basin the variation of the alkalies is still quite gradual, but the ratio of the soda and potash molecules varies from about 2 : 1 to nearly 1 : 1. There is a greater proportion of potash, and the total percentage of alkalies is higher. In the special group of dike rocks from the Absaroka range and the exceptional flows associated with them the molecules of potash are more nearly equal to those of soda, and the total amount of alkalies is still higher than in the previous groups. It is known that in the regions north and east of this the amount of alkalies increases very considerably in certain groups of rocks, but no systematic chemical study has been made of them.

Comparing the groups of analyses just given with those of Vesuvius and its vicinity, a marked difference is at once observed. In the latter group the variation of the molecules of the alkalies is more irregular, though they maintain a generally uniform proportion, which is indicated by their nearly horizontal position in the diagram; but the potash is more uniform than the soda, which increases from the less silicious to the more silicious end of the series. The ratio between the potash and soda varies from nearly 4 : 1 to 4 : 7. In general the potash is much greater than the soda, which is the distinguishing characteristic of this group. In the four groups already mentioned the alumina, though quite variable, maintains a high average except in certain parts of the rocks in the Absaroka range and Crandall group.

In the rocks of the Etna group the variation of the alkalies is quite irregular, and the soda is in excess of the potash, the ratio of the soda to potash ranging from 8 : 1 to about 1 : 1, while in the more silicious rocks from Pantelleria the alkalies are more uniform, and the soda is greatly in excess of the potash, varying from about 2 : 1 to 5 : 1, the total amount of alkalies in the Vesuvian and Pantellerian groups being much greater than in the rocks of the Yellowstone Park region. In the rocks of Etna and Pantelleria the alumina varies greatly for a slight range of silica, in both groups falling rapidly toward the more silicious end of the series.

In that from Pantelleria it is apparent that the alumina is much less than the alkalies, and that a large part of the alkali molecules must enter into a non-aluminous mineral molecule.

The chemical characteristics of these groups of rocks are also expressed by the behavior of the other essential oxides. The reciprocal nature of the two oxides of iron is noticeable in all of the groups and has been discussed in another connection.* In the third diagram of each series the iron is represented as ferrous oxide.

In the group from Electric peak and Sepulchre mountain the similarity in the variations of the lime, magnesia, and iron oxide is very marked, there being a great decrease of the molecules of these elements from the less silicious to the more silicious end of the series. The magnesia is slightly more variable than the other two, and the ferrous oxide is less abundant than the lime, though this relation is varied in the rhyolitic lavas at the end of the diagram. In general, these oxides are in nearly equal proportions as indicated by their close grouping in the diagram. In the Crandall group the relation between these oxides is like that just mentioned, the iron is slightly less than the lime, and the magnesia is still more variable, especially at the basic end of the series, and this is even more pronounced in the Absaroka group. In the first group the average of all of these oxides is higher than in the second and third groups for rocks of like percentage of silica. The generally uniform variation and diminution of these molecules, together with the highly variable behavior of the magnesia, are characteristic of these groups of rocks.

In the group from Vesuvius and vicinity the variations are greater and the relative proportions are not so nearly the same. This is shown by the greater spread of the lines. The lime is the most variable and is considerably greater than

* Papers on the rocks of Electric peak and Sepulchre mountain already cited. Twelfth Annual Report U. S. Geol. Survey, p. 630. Bull. Phil. Soc. Washington, vol. 11, p. 212.

the magnesia. Both of these show a marked decrease in amount from the less silicious to the more silicious end of the series, while the iron oxide maintains a more uniform position, decreasing only slightly, and being considerably in excess of the other two oxides in the more silicious rocks of the group.

In the Etna group the most noticeable feature is the quite uniformly high position of the lime and iron and the increase of magnesia from the less silicious to the more silicious end of the series, and the greater variation in the magnesia. It is in general much lower than the other oxides, and the lime is in most cases the highest.

In the silicious group from Pantelleria the lime and magnesia are comparatively constant, the lime being fully double the magnesia, while the iron oxide is greatly in excess, and increases toward the more silicious end of the series. In the last two groups the noticeable decrease in alumina is accompanied in one case by an increase in magnesia and in the other by an increase in iron oxide.

It would seem that these illustrations of the different behavior of the molecules of the essential oxides in various groups of rocks are sufficient to establish the consanguinity of these groups upon a chemical basis. It is perfectly evident that the correctness of such conclusions rests on the completeness and accuracy of the chemical analyses, and it will be admitted that the work of the chemists connected with the U. S. Geological Survey commands the utmost confidence. Too much stress cannot be laid on the absolute necessity of thorough and correct chemical investigation of the rocks of any region where the freshness of the rocks warrants the undertaking. Moreover, the worthlessness of inaccurate analyses is equally apparent. It is undoubtedly true that the confusion brought about by incorrect chemical analysis, often of unfit material, has seriously blocked the progress of petrology for years, so that the chemical development of the science is still in the future.

The chemical individuality of geographical groups of rocks

explains the association of various kinds of volcanic rocks, such as those in the regions selected for discussion, and also shows the error of attempting to correlate all kinds of rocks on a simple basis of silica percentages. Thus the natural series of volcanic rocks in the Yellowstone Park, arranged according to the approximate percentages of silica, is:

Silica percentages.	
48–53	Basalt.
55	Hypersthene-basalt.
58	Pyroxene-andesite.
61	Hornblende-pyroxene-andesite.
65	Hornblende-mica-andesite.
68	Dacite.
70–75	Rhyolite.

The series of exceptional dike and flow rocks of the Absaroka range is:

47–53	Leucite-orthoclase-basalt (phonolitic basalt).
53–56	Basaltic-trachyte.
57–61	Trachyte.
69	Rhyolitic-trachyte.

The series of rocks from Vesuvius and Ischia is:

46–55	Leucitophyre.
55–62	Trachyte.
69–71	Rhyolite (Ponza).

The series from Pantelleria is:

49	Basalt (doleritic).
50–56	Phonolite.
61	Augite-andesite (with cossyrite).
67–70	Pantellerite.
70	Rhyolite.

In these series even those rocks to which have been given the same names differ from one another very considerably

in mineral composition, as they do in chemical composition, and their connection with the other rocks of the series in which they belong is much more intimate than their supposed connection when placed in an artificial group in accordance with present methods of classification.

GEOLOGICAL EVIDENCE OF CONSANGUINITY.

The *geological occurrence* of igneous rocks indicates their consanguinity, and was the first means by which it was recognized. The constant recurrence of particular series of rocks, often with a certain order of eruption in different localities, and the frequent occurrence of such series at neighboring centers of volcanic activity, sometimes with a repetition of the whole or a part of the series, would be enough to justify the belief that there was a definite connection between the members of a group; but in regions like those of the denuded volcanoes of Electric peak and Crandall basin the physical connection between the varities of rock is made evident. The breccias and lava streams, with intersecting systems of dikes and intrusive sheets joined to a complex core, combine the diverse modifications of the whole group of rocks into a geological unit.

It is to be remarked that the term "group of rocks" is necessarily somewhat indefinite, since it may be applied to all of the rocks derived from one center of eruption, or to all those of a number of centers in one region of country which can be shown to have been connected and contemporaneous.

There will be found, most probably, a gradual transition from a region of one character of igneous rocks to that of another, so that no line can be drawn between them. Such a transition is indicated in several places, from the rocks of the Great Basin to those of the belt east of the Rocky mountains. It is hoped that more will be learned of this eastern belt of alkali rocks, and that the geographical distribution of the various groups of volcanic rocks all over the world

will be more thoroughly investigated, in order that we may discover whether there exists any connection between the different types of rock groups which occur over great areas of the globe and the geological history of those regions.

DIFFERENTIATION OF MOLTEN MAGMAS.

GEOLOGICAL EVIDENCE.

It having been demonstrated that the igneous rocks in one region, or about one center of eruption, which belong to an era of volcanic activity, exhibit characteristics which serve to identify them as belonging to a connected group; and it having been shown that they pass into one another by gradations of chemical composition, and are also connected physically by their geological occurrence in many cases, it remains to be discovered how these relationships came into existence or what is the origin of the variation among the rocks of one group.

In a great many regions where the geological occurrence of all of the igneous rocks belonging to one eruptive epoch has been thoroughly studied, a definite order of eruption has been observed. The result of extensive observation in many parts of the world has been summed up by von Richthofen in his well-known order of succession for massive eruptions. In consequence of the subsequent development of microscopical petrography, the understanding of the terms in which he expressed this law has been modified, but when the rocks to which the terms were applied are kept in mind it still has its value as a broad generalization, especially when it is applied to the regions particularly studied by von Richthofen, namely, Hungary, China, and the Great Basin and Pacific coast of the United States.

Von Richthofen's law of succession does not apply to all regions and occurrences of volcanic rocks, especially to eruptions from a center, as in a volcano, where frequent repetitions of whole or partial series occur. Nevertheless it ex-

presses a natural order of eruption of frequent occurrence, which his own theory of the origin of igneous rocks failed to explain satisfactorily.

In a number of localities studied by the writer, especially in the region of the Yellowstone Park, the order of eruption, which may be repeated more than once in some localities, is: andesite of mean composition, including hornblende-andesite and hornblende-mica-andesite, followed by eruptions of more basic andesite and basalt, and more silicious andesite and dacite, and by basalt and rhyolite and basalt, which may be modified in many ways in particular localities. Thus in various parts of the west, especially in the Sierra Nevada, we are told that rhyolite is usually succeeded by andesite.* The general succession is from a rock of average composition through less silicious and more silicious ones to rocks extremely low in silica and others extremely high in silica—that is, the series commences with a mean and ends with extremes.

This law of succession, expressed in its most general terms, is of very wide application and holds true for all the localities studied by the present writer.

It has been shown by Judd† to be the order of eruption of the lavas of the Lipari islands, which began with rocks of intermediate composition, and has reached the stage where rhyolite and basalt are being thrown out.

It is to be remarked that constant exceptions to the general law of succession are observed. Many and possibly all of them arise from the fact that only a part of the whole series of events may take place in one limited locality or center of eruption, as at Electric peak and at the volcano of Crandall basin. The whole series of eruptions in this region embrace all of the igneous rocks of the region belonging to the period of volcanic activity, and the volcanoes mentioned

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* Turner (H. W.) Mohawk Lake Beds. Bull. Phil. Soc. Washington. 8°. Washington, 1892, vol. 11, p. 389.

† Judd (J. W.) Contributions to the Study of Volcanoes. Geol. Magazine. 8°. London, 1875, *new series*, vol. 2, p. 60.

represent localizations of the actions during certain parts of this period of volcanic activity.

In the two anniversary addresses of Sir Archibald Geikie, which he delivered as president of the Geological Society,* an account is given of the history of volcanic activity in Great Britain from pre-Cambrian times to the Tertiary—a history of the most remarkable series of volcanic events yet recorded. The great variability of the rocks in composition and the recurrence of similar series of varieties has led Geikie to conclude that the rocks of a natural group or of a period of volcanic activity have resulted from changes that have taken place in the magma during such a period; but in considering the nature of the changes he states: "With the important exception of the Snowdonian region and possibly others, we find that the earlier eruptions of each period were generally the most basic, and that the later intrusions were most acid."

Since this order of succession is in conflict with the general law of Von Richthofen for Tertiary and recent volcanic eruptions, and also with the observations of the present writer, and since it is important to learn whether this law is applicable to whole periods of eruption at various geological epochs, the writer has taken the liberty to analyze the order of succession so far as it has been made out in Great Britain and has been given in the addresses cited. This has been done with the following result, the order of succession being indicated by Roman numerals:

LEWISIAN.

I. Dolerites and basalt.
II. Peridotites and picrites.
III. Granite and "syenite."

(Basic rocks, followed by highly basic and highly acid rocks.)

DALRADIAN.

Diorite, epidiorite, hornblende-schist, with basic and acid lavas.

(Intermediate rocks, followed by basic and acid ones.)

* Geikie (A.) Quart. Journ. Geol. Soc. 8°. London, vol. 47, May 1, 1891, and vol. 48, May 2, 1892.

URICONIAN.

I. Acid lava, breccia, and tuff (felsite and rhyolite).
II. Basic sills.

CAMBRIAN.

Llyn Padarn.—I. Quartz-porphyries, felsites or rhyolites.
II. Andesite, tuff, and breccia.
St. David's.—I. Diabase and felsite (tuffs, mixed).
II. Olivine-diabases, and porphyrites.
III. Granite and quartz-porphyry.
IV. Diabase.

SILURIAN

Arenig.—I. Andesite (tuffs and conglomerate) with quartz-andesite, rhyolite, or trachyte becoming more acid.
II. Greenstone, dolerite, (sills), eurite and granophyre.
III. Olivine-diabase (dikes).
(Volcanic action possibly continuous from Arenig into Bala.)
Bala—Lleyn.—I. Pyroxene-andesite.
II. Felsites (lavas and tuffs becoming more acid.)
III. Andesite.
Snowdon.—(Altering lava flows of gray andesite or trachyte and felsite.)
IV. Diabase (sills).
Y-foel-frâs.—I. Felsite.
II. Agglomerate and tuff (of what?)
III. Diabases and andesites.
IV. Granophyre.
V. Diabase.
Anglesey.—I. Felsitic tuff.
II. Dolerite dikes.
Lake district.—I. Andesites, approaching basalt toward the last.
II. Felsite (rhyolite toward the top.)
III. More basic, apparently basalt.
(Dikes of felsite, quartz-porphyries, diorites, and mica trap, (?) minette.)
Scotland.—I. Porphyrite (lava and agglomerates).
II. Felsite (tuff).
III. Gabbro and serpentine (sheets).

OLD RED SANDSTONE.

Pentland Hills.—I. Porphyrites and diabases (bedded).
II. Felsite (filling vent as tuff and breccia).
III. Felsite, porphyrite, and dolerite (dikes).
(Also dikes of felsite, minette, lamprophyre, vogesite, and other varieties—that is, acid and very basic rocks.)
Tuffs of acid material alternate with flows of diabasic and andesitic lavas.

CARBONIFEROUS.

(The plateaux in general precede the puys.)

(A) *Plateaux.*—I. Chiefly porphyrites (variable composition), more basic rocks intercalated near the bottom, the ultra basic possibly intrusive (later).

II. More acid lavas (sanidine trachyte), preceded by some basic flows.

Garlton Hills.—I. Basic lavas (olivine-basalt, and (?) picrite) and porphyrite (andesite).

II. Trachyte, with occasional flows of andesite.

(B) *Puys.*—I. Basalts mostly (lavas) (some with olivine, some without).

II. Porphyrite occasionally (lavas), (with intrusive sheets, necks, and dikes of same kinds of rock).

TERTIARY.

I. Basalts, with some trachytic tuffs interbedded, cut by gabbros.

II. Trachytes, felsites, quartz-porphyries, granophyre, and granite.

III. Basalt.

From the foregoing crude analysis it appears that there are frequent alternations of more silicious and less silicious lavas, and that with few exceptions the series are closed with basic lavas, usually in dikes or sills. Moreover, the first eruptions are seldom the most basic, but are more frequently rocks of intermediate composition. This is also the case in certain districts of Tertiary rocks, according to Judd,* who has shown that in the Western isles of Scotland there occurs beneath the basalts an older accumulation of lavas, consisting of altered andesites, together with "rhyolites, dacites, and sanidine-trachytes, intercalated with which are a few basalts." The history of volcanic action throughout the geologically vast range of time in Great Britain, though not yet completely deciphered, appears to accord with what has been observed in many regions of Tertiary and recent activity. The order of eruption indicates a variation in most cases from intermediate to more and more extreme varieties of

* Judd (J. W.) Quart. Journ. Geol. Soc. 8°. London, 1874, vol. 30, p. 236, and *ibid.*, 1890, vol. 46, pp. 342–385.

magma, the outflows alternating with one another without a rigid order of sequence. The descriptions so far given seem to indicate that the groups of rocks erupted at these successive periods had the same general range of composition—that is, the magma from which each group originated had the same or very similar chemical compositions.

But when it is remembered that most of the ancient lavas are considerably decomposed or metamorphosed, it is hardly to be expected that their original chemical composition has remained unaltered; so that it may always remain a matter of uncertainty. When this alteration is taken into account, and also the fact that the full sequence of events may not be represented in a given locality, either because of incomplete exposure of the rocks or of their partial erosion, when subaërial deposits, it seems to the writer that the general law of sequence as stated in this paper will be found to hold good for the volcanic rocks of all epochs.

The Devonian eruptive rocks of the region of Christiania, as briefly described by Brögger, are not entirely in conflict with this law of succession, although they are said to have commenced with extremely basic varieties. It is stated that there are alternations of more and of less silicious varieties, the general change being toward highly silicious ones, until the final eruptions, which were basic.

From the mode of occurrence, then, of the rocks of a complete group, it is evident that as the eruptive action in the district proceeded the rocks became more highly differentiated. This is more quickly appreciated by studying the chemical composition of the various members of a rock group.

CHEMICAL EVIDENCE.

Before taking up the study of the chemical composition of the rocks it is necessary to call attention to the fact that the tables of chemical analyses and diagrams of molecular proportions are arranged according to the silica percentages of all of the rocks of each group, irrespective of the order in

which they were erupted. They express only the variation from one chemical extreme to the other, considering the group as a whole. The introduction of a third variable, if it were possible to fix the order of eruption for each rock analyzed, would necessitate a third set of coördinates, involving cubical projection, which would not be feasible for so complicated a set of lines.

It is also to be observed that the lavas which constitute the rocks whose order of succession is from intermediate to extreme varieties are not bodies with a particular or uniform composition in any case where their volume is large. They are variable within certain limits, and the limits of a body of later eruption may overlap those of the previous body to a greater or less extent. Thus the statements about to be made regarding the changes in the composition of rocks of different eruptions must be understood in a general sense.

The rocks of intermediate composition, which form the earliest eruptions in the Yellowstone Park region, have a range of small extent, of perhaps 6 per cent. of silica. In several localities the oldest rocks contain 61.50 per cent. of silica, and possess the other elements in almost the same proportions as the average of 64 analyses of the Yellowstone Park rocks, as it has been reckoned by Prof. F. W. Clarke * in a paper on "The Relative Abundance of the Chemical Elements," the silica in the average being 61.89 per cent. From this mean the successive eruptions through alternating and interlocking series pass into basalts with 47 per cent. of silica, and into rhyolites with 75 per cent. of silica.

In particular localities, as at Crandall basin, the whole of the great series may not be represented as eruptions from one center, but a partial series may occur in which the same relations exist with certain modifications. Thus the earliest eruptions from the Crandall center were more basic than the general average of all of the rocks of the Yellowstone region. They probably ranged from 58 to 53 per cent. of silica. As

* Clarke (F. W.) Bull. Phil. Soc. of Washington. 8°. Washington, 1892, vol. 11, pp. 131–142.

the eruptions approached a climax at this center, part of them became more and more silicious, passing the general mean of 61 and reaching 64, and as an extreme 71.62 per cent. of silica; part became less and less silicious and passed through 50 to 48 per cent. of silica. It is to be noted that the volume of these extreme varieties of lava is much smaller than that of the intermediate lavas at this center.

When the changes in the other essential oxides of these rocks are considered in connection with the variations in silica, it is evident that as the eruptive action about this center proceeded the chemical character of the lavas erupted became more and more differentiated. The same can be shown for other great series of eruptions; so that it may be stated as a general law that *the variation in the composition of igneous rocks, which constitute a series of eruptions at any volcanic center, is the result of the chemical differentiation of some intermediate magma.* The composition of the intermediate magma may be different in different centers of eruption and in different regions, and it will be shown subsequently that the intermediate magma of any particular center may itself be the result of a differentiation of a more ancient magma or of a primary uniform magma, if such a thing can be shown to have existed.

Having demonstrated by the geological relations of the igneous rocks of any one region that their variation of composition must be due to a chemical differentiation of an intermediate magma, it remains to discover what is the character of this differentiation and what were the causes which brought it about.

CHARACTER OF THE CHEMICAL DIFFERENTIATION.

In order to discover what is the character of the differentiation which has produced the variations in the rocks of any district, it is necessary to study still more closely the chemical character of the rocks. In the tables of analyses already presented the original form of the analyses has been preserved and the molecular proportions of the essential

oxides have been inserted in a different type. Since it is the relative proportions of these oxides in each rock which are to be compared with one another, it is not necessary to reduce them by some common factor in each case so as to bring the sum totals to 100. In fact, it is more accurate in most instances to compare those in different rocks, as they have been determined to exist in the rocks, than to assume a theoretical proportion by omitting from the analysis a number of accessory ingredients and then readjusting the proportions of the essential ones. Moreover, it seems unnecessary to consider the constituents in the metallic state, since in all of the rock-making minerals they exist in the same state of oxidation, with the exception of iron, and the molecular proportions have the same relations, whether they are discussed as oxides or metals. In either case the results reached by their consideration would be the same.

Absence of stoichiometric proportions.—Referring to the six tables of analyses already given, which represent the chemical composition of various members of six groups of geologically related rocks, some of them in the Electric peak and Crandall basin groups representing different facies of one rock body, we observe the irregular variability of the molecular proportions of the constituent oxides.

All attempts to calculate a stoichiometric relation between them is fruitless. It is possible, however, to express in very general terms certain relations which appear to exist between the constituents of the rocks of one group; but other groups of rocks can be found in which the relationship is different. How indefinite such relationships actually are will appear when the diagrams of molecular proportions are examined in detail.

Some of the generalizations for the groups of analyses under discussion are as follows:

In those groups in which the alumina maintains a generally uniform proportion and the alkalies increase gradually with the silica, the magnesia, lime, and iron oxide decrease rapidly as the silica increases.

In groups in which the last mentioned oxides maintain a nearly uniform proportion, the alumina decreases as the silica increases, and the alkalies may or may not do the same. In the Etna group they decrease with the alumina, while at Pantelleria they remain nearly constant.

The alkalies maintain no fixed relation to one another, though they generally behave in somewhat the same manner. Their relative proportions in different groups of rocks is quite varied and characteristic.

Owing to the uncertainty attending the determination of the oxidation of the iron in rock analyses, little reliance can be placed on most determinations of it; but in the very carefully executed analyses of the rocks from the region of the Yellowstone Park the different oxidations of the iron are more than usually accurate. In these groups of rocks the reciprocal behavior of the ferrous and ferric oxides is one of the most marked chemical features. They are undoubtedly connected by some definite relationship. When considered separately they exhibit no connection with any other constituent, but when combined as ferrous oxide they fall in place with the lime and magnesia; so that it seems highly probable that during the differentiation of the magma all of the iron existed in the ferrous condition and was acted on as ferrous iron, and that subsequently it was in part more highly oxidized, so that the more ferric oxide was produced, the less ferrous remained.

As just remarked, when all of the iron in the rocks is reduced to ferrous oxide it is found to vary in a general way with the lime and magnesia; but these constituents do not always vary to the same extent. In the three groups from the Yellowstone region the magnesia is the most variable oxide, attaining its highest degree of variation in the basic rocks of Crandall basin and Absaroka group, while the iron oxide is the least variable. The same is true for the Etna group, though in this case the magnesia is subordinate in quantity to the lime and iron oxide. In the Vesuvian group the lime is most variable and the iron oxide the least

so, while at Pantelleria the iron oxide is the most variable and the predominant bivalent element.

The most marked instance of discordance is between magnesia and alumina; so that it may be stated in general that as the magnesia increases, the alumina decreases; to this there are numerous exceptions. Moreover, they do not vary to the same extent. The most notable instance of this is in the Crandall group of rocks. The last mentioned relationship shows that in general as the aluminous molecules increase in number, the magnesian molecules decrease; but this relationship is not so definite that it may be expressed in terms of simple mineral molecules. One would expect from this that as the magnesian minerals increase in abundance in a rock, the aluminous minerals must decrease. In a general way, this is undoubtedly true; but it is not correct to assume that the aluminous minerals are wholly feldspathic, or that the alumina is always present in combination as a feldspathic molecule. Aside from any knowledge of the actual presence of alumina in certain ferromagnesian minerals that occur in rocks, it may be shown by such diagrams as that of the group of rocks from Electric peak that there are instances in which the alumina molecules exceed those of soda, potash, and lime combined; so that the alumina must enter into some other compound molecules than the feldspathic.

Chemical independence of the essential oxides.—It has been shown that molten magmas which have similar chemical compositions crystallize into different associations of minerals according to the conditions under which they solidify,* or, more correctly, through which they pass during crystallization; so that the chemical constituents of a molten magma are capable of entering into different molecular combinations according to circumstances. From this it must be concluded

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* Roth (J.) Gesteinsanalysen in tabellarischer Übersicht, etc. 4°. Berlin, 1861, p. xxi.

Iddings (J. P.) Bull. Phil. Soc. Washington. 8°. Washington, 1892, vol. 11, p. 217; also Twelfth Annual Report U. S. Geol. Surv. 8°. Washington, 1892, p. 656.

that they do not exist in a molten magma in fixed molecular compounds that correspond to definite mineral molecules, but are in effect uncombined and free to form such combinations as circumstances may impose.

From the great variability in the proportions of the simple oxide molecules in the different varieties and facies of rocks belonging to one group, and from the absence of all stoichiometric ratios between these oxides, it is equally evident that in the process of differentiation the simple oxide molecules shift about independently of one another to a great extent, though certain tendencies toward association must not be overlooked and the little understood force termed "mass action" must be taken into account.

Since the foregoing conclusion was written Prof. F. W. Clarke has called the writer's attention to the steadily accumulating evidences that are leading chemists to the opinion that many salts when in solution are dissociated to a greater or less extent.

The paper by Arrhenius* on the dissociation of matter dissolved in water notices the exceptions that are found to exist to the law of osmotic pressure as stated by van't Hoff, and suggests that they correspond to the exceptions to Avogadro's law of gaseous pressure, which are due to the dissociation of gases. They are both additive in character, and Arrhenius argues that in such cases the salts in solution are more or less dissociated.

Following this line of argument it is found that a salt in solution may be partly dissociated and partly combined, and that the extent of dissociation is greater as the solution is more dilute, and that it varies with the nature of the salt, and is most noticeable with strong bases and strong acids. It is also pointed out that the lowering of the freezing point of a solution is an additive effect which indicates a state of more or less dissociation.

This view of the character of solutions appears to be in

* Arrhenius (S.) Über die Dissociation der in Wasser gelösten Stoffe. Zeitschr. phys. Chemie. 8°. Leipzig, 1887, pp. 631–648.

accord with the behavior of molten magmas in the process of differentiation, as well as in that of crystallization. Their behavior as eutectic substances, or those in which the point of solidification is below that of their components, has been pointed out elsewhere.

The differentiation of molten magmas is not according to stoichiometric proportions and is therefore not a mineralogical differentiation. Other arguments which might be brought against the idea that the differentiation was a process of mineral separation are: the wholly uncrystallized condition of many lavas at the time of their eruption, and of most coarsely granular rocks previous to their arrival in the conduit in which they have solidified, and the consequent crystallization of the minerals *in situ*, and the comparatively rapid growth of all phenocrysts in porphyritic rocks.

The character of the differentiation of molten magmas is therefore that of a chemical separation, affecting the elementary oxides of the constituent elements. The question naturally following this conclusion is, How has such a differentiation been brought about?

CAUSE OF THE DIFFERENTIATION OF IGNEOUS MAGMAS.

Molten magmas as solutions.—It was pointed out in 1861 by Bunsen* that the order of crystallization of minerals in igneous rocks indicated that molten rock magmas are solutions of silicate compounds which are liquid at high temperatures. This fact has been enlarged upon by Lagorio,† who considered molten magmas as saturated solutions of silicates, which are capable of carrying different amounts of different oxides. The result of his investigations led him to consider the solvent as a silicate with the composition of $(K, Na)_2O, 2\ SiO_2$.

The character and probable behavior of molten magmas

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*Bunsen (R.) Zeitschr. deutsch. Geol. Gesell. 8°. Berlin, 1861, vol. 13, p. 62.

†Lagorio (A.) Tschermak's min. petr. Mitth. 8°. Vienna, 1887, vol. 8, pp. 437 and 508.

when considered as saturated solutions or alloys of silicate molecules has been discussed by the writer in a paper "On the crystallization of igneous rocks,"* when the influence of physical conditions on the saturation and crystallization of the compounds was especially dwelt upon.

The analogy between alloys and solutions is so close that there seems to be no essential difference between them, and they may be considered as synonymous.

That one particular compound in the mixture of a magma should be considered the solvent for the others in all cases seems to the writer to be contrary to the evidence furnished by the results of crystallization which are exhibited by the microstructure of rocks; for, if it is necessary to consider in each instance that a certain portion of the magma acts as the solvent for those compounds which separate first from the magma when it crystallizes into rock, then a study of the order of crystallization of the minerals in rocks proves that different compounds have played the part of solvents in different magmas; and since the general character of these compounds is the same in all rocks, it is evident that the behavior of any one of these compounds as solvent for the others depends on the conditions of their association—that is, chiefly on their relative amounts, or on their mass. A few illustrations of extreme cases will be sufficient.

In the highly silicious and feldspathic rocks, when the crystallization is coarse enough to permit all of the minerals to be identified, it is found that the last to solidify was a mixture of alkalies, alumina, and silica in such proportions that it separated on crystallizing into alkali feldspar and quartz. Since the crystallization of these two minerals is in many cases synchronous, the solvent of the other compounds of the rock must have been a mixture of the constituents of these two. In certain phases of the gabbro occurring in the granular core in the Crandall region, the last mineral to crystallize was orthoclase, which formed a

* Iddings (J. P.) Bull. Phil. Soc. Washington. 8°. Washington, 1889, vol. 11, pp. 65–113.

cement for the other constituents of the rock. In these instances the solvent must have had the composition of an alkali feldspar. In the remarkable leucite rock from the Stinking Water canyon, which consists of abundant augites and olivines in a groundmass of orthoclase and leucite, the solvent consisted of alkalies, alumina, and silica in such proportions that there was too little silica to convert all of the alkalies and alumina into alkali feldspar, and it was necessary for the mixture to split into alkali feldspars and leucite, the alkalies and alumina entering into both these minerals in the ratio of 1 to 1, but the ratio of the silica molecules being 6 in feldspar and only 4 in leucite.

In many kinds of gabbro and dolerite, where an ophitic structure has been developed, the last mineral to crystallize was pyroxene, and the solvent of the other constituents must have had the composition of pyroxene. While in hornblende-picrite, with large poikilitic hornblendes, the composition of the solvent must have been that of hornblende.

Thus, on the supposition that the compounds in a molten magma exist in the form of definite mineral molecules, it is evident that no one of them can be considered as universally playing the rôle of solvent for the others. But, from the arguments already presented, it appears that the compounds in a molten magma do not exist as definite mineral molecules, nor even as fixed silicate molecules, but that they behave as simple oxide molecules, capable of arranging themselves in different associations according to physical circumstances; hence it is not correct to assume that any definite compound is the solvent in a molten magma, but that the rôle of solvent is assumed temporarily by different compounds, according to the relative abundance of the elementary constituents and the physical conditions under which the magma exists.

Soret's principle.—With this conception of the flexibility of the solution of molten magmas, it is in order to inquire by what means their differentiation into magmas of very variable composition can have taken place. One answer

has been suggested—first by Lagorio in explanation of an extremely local differentiation between the center and sides of some dikes, and afterwards by Teall as a possible explanation of the differentiation of large bodies of magma. It is the application of the principle demonstrated by Soret, who found that if two parts of a solution of any salt be kept at different temperatures there will be a concentration of the salt in the cooler part of the solution. This is in accord with the law of osmotic pressure in dilute solutions, which van't Hoff has shown is analogous to the law of gaseous tension.*

In the case of certain dikes Lagorio pointed out that the ferromagnesian elements had been partially concentrated along the sides of the dikes. The same thing has been observed by the writer in one of the dikes in the granular core of the Crandall volcano. The more silicious character of the walls of certain dikes at Santorin has been explained by Fouqué † by the supposition that the glassy sahlband was formed by the chilling of the first portion of the magma erupted through the fissure and its adhesion to the walls of the fissure, the central part of the dike being composed of a later portion of the magma. He further suggests that the upper part of a body of magma may be lighter than the lower portion, and may consist of a greater amount of silicious fluid magma, while the lower part carries more crystals.

Chemical equilibrium and pressure.—It is in accord with the principles of thermo-dynamics as pointed out by Gouy and Chaperon, ‡ to which allusion is made in Vogt's paper, that there should be a change of density in a solution from the upper surface downward, and that the concentration varies proportionally to the depth. They conclude that

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* Van't Hoff (J. H.) Die Rolle des osmotischen Druckes in der Analogie zwischen Lösungen und Gasen. Zeitschr. f. Phys. Chemie. 8°. Leipzig, 1887, vol. 1, p. 481.

† Fouqué (F.) Santorin et ses Éruptions. 4°. Paris, 1879, p. 304.

‡ Gouy (—.) and Chaperon (G.) Sur la concentration des dissolutions par la pesanteur. Ann. de Chimie et de Physique. 8°. Paris, 1887, 6th Ser., vol. 12, pp. 384–393.

the effect of concentration from this cause in dilute solutions will be very slight for a depth of 100 meters, but should be more noticeable in concentrated solutions, and from the nature of the case would take place with great slowness.

Hence it is possible that in a very large body of molten magma the chemical equilibrium in different parts of it may be sufficiently diverse to cause a shifting of the molecules and the consequent differentiation of the magma; but, while the correctness of this hypothesis is highly probable, we are not yet in possession of the experimental facts which might establish it. Moreover, the absence in nature of decisive evidence of the influence of pressure on the crystallization of igneous magmas suggests that its effect is relatively slight as compared with the influence of temperature on crystallization, and the often noticeably localized character of the differentiation of igneous magmas makes it probable that the bodies of magma in which it has taken place were not large enough to have experienced great diversity of pressure in different parts of them; so that while differences of pressure may influence the chemical equilibrium of these magmas, their chemical differentiation is more likely to have been controlled by differences of temperature, which, it is readily imagined, may vary considerably in different parts of a magma which occupies an irregularly shaped cavity within the earth's crust. Both the shape of a body of molten magma and the nature and conductivity of the enclosing rocks will affect the temperature of different parts of it to a very considerable extent.

From the foregoing it may be concluded that the molecular concentration of particular constituents of molten magmas in the cooler parts of inclosed bodies of magma is a sufficient cause for their differentiation.

THE PROCESS OF DIFFERENTIATION.

From the independence of the elementary oxide molecules from any fixed combination within a molten magma and the

fact that there are at least six essential oxides and several less abundant oxides involved in the composition of the magma, and from the undoubted influence of mass action in determining the behavior of the elementary molecules, it is evident that the results of the process of differentiation may be highly complex.

A study of the diagrams of molecular proportions in the groups of rocks from the Yellowstone Park region shows the absence of any persistent law of variation.

In the Electric peak group, with some of the rhyolitic rocks attached, the soda and potash vary but slightly from the middle of the group to the extremes, or from one end of the series to the other. They remained very nearly constant throughout the differentiation. In many cases they vary in opposite directions; so that the total amount of alkalies is less variable than each separately, and there is a tendency for them to replace one another; but in some instances they vary independently, and in others they vary in the same direction. No distinct relationship can be made out between the alkalies and alumina, although they all maintain considerable constancy and are in this sense alike. In the group of rocks from Crandall basin the same general statements may be made concerning the alkalies and alumina. In this group the variation of the alkalies is even less than in the group from Electric peak; but in the group of exceptional dike rocks and surface flows, which are connected with the Crandall basin rocks and are represented in diagram 3, the variation of the alkalies and alumina are greater, and in general are in like directions, except at the less silicious end of the series, where the alkalies vary in opposite directions.

In the same manner an examination of the magnesia, lime, and ferrous oxide shows no persistent relation between them, though in general they vary in like directions and decrease rapidly with an increase of silica. It is also evident that in these groups of rocks the ferrous oxide is the least variable constituent and the magnesia the most variable, lime being intermediate. Moreover, these oxides are much more

variable than the alkalies and alumina. For this reason the more silicious rocks of this region are less variable in composition than the less silicious rocks.

The process of differentiation has separated the average magma into partial magmas, some of which are high in silica, with much alumina and alkalies and little magnesia, lime, and iron oxide; and into others low in silica, with nearly as much alumina and slightly less alkalies, but abundant magnesia, lime, and iron oxide; and into others low in silica, some of which are unusually low in alumina and relatively high in alkalies, and extremely high in magnesia and lime and moderately high in iron oxide, while some are high in alumina and alkalies and relatively low in magnesia, lime, and iron oxide.

Thus there is a tendency to concentrate the silica into one part of the magma where there is a greater amount of alkalies and an average amount of alumina, and to concentrate the magnesia, lime, and iron oxide in another part where the alumina is about the average and the alkalies are somewhat lower. There is a further tendency toward the end of the differentiation, which has given rise to the exceptional dike rocks and flows, for the portion of the magma low in silica to separate magnesia, lime, and iron oxide from alumina and alkalies producing rocks rich in ferromagnesian silicates and poor in feldspar, corresponding to certain lamprophyric rocks—in this case minettes and leucite-basalts; and also producing rocks rich in alkali feldspar and poor in ferromagnesian minerals, corresponding to syenite-porphyry, or trachyte.

The exceptional dike rocks and flows of the Absaroka range, which will be described in a later part of this paper and still more fully in another place, represent the last phase of activity of the centers from which the older basaltic rocks of this range were erupted. They grade into these basaltic rocks and are connected with them in a geological group, though for a special purpose they have been considered separately. Grouped by themselves, they possess the pecu-

liarity of having more potash than the main portion of the group or the rocks of Crandall basin. The soda is about the same in both groups, and so is the alumina in the more silicious end of the series.

This group of exceptional rocks has a great range of silica, including intermediate amounts. At the least silicious end of the series there is a great diversity in alumina and magnesia for rocks with nearly the same percentage of silica. With all this variability of composition, these exceptional rocks are united by their association in the field, and by the time of their eruption, as well as by a high percentage of potash, into what might be called a sub-group. They represent the differentiation of a magma which is connected by its geological occurrence and by petrological characters with the less potassic rocks of the main group. It must be the result of a differentiation of the magma of the main group, for it is of small volume and is followed by immensely greater masses of magmas of the normal type for this region. The more potassic magma is not confined to one center of activity, but occurs at numerous centers and represents a modification of the magma which took place at each of these centers.

This proves the existence of a number of successive orders of differentiation which undoubtedly took place on a larger and smaller scale. Thus the separation of a large body of magma into a series of normal partial magmas, also produced a partial magma with an increased percentage of potash; this was followed by a more complex differentiation, which produced the extremely magnesian varieties and the highly feldspathic varieties of the potassic magma. This closed one phase of eruptive activity, and the more voluminous eruptions that followed reverted to the average magma of the region.

The difference between the chemical composition of the main group and sub-group in this instance corresponds to that which exists between some geographical groups, so that the inference may be drawn that the cause of the difference

is the same in both cases—that is, it is the result of differentiation.

RESULTS OF THE DIFFERENTIATION OF IGNEOUS MAGMAS.

The chemical differentiation of igneous magmas, which appears to be due for the most part to so simple a cause as an inequality of temperature in different parts of the magma, leads to an endless series of variations in the composition of so complex a mixture as the magma of eruptive rocks and accounts for many of the obscurer relationships of these rocks.

The application of this principle to the explanation of some of the phenomena of eruptive rocks will be pointed out. It explains the chemical variability of large bodies of rocks or the development of chemical facies of rock bodies and the changes in composition of lavas which follow one another through the same conduit or are associated together at one center of eruption. And in a long period of volcanic activity the continued differentiation of a particular body of magma will lead to the production of extreme varieties of composition, differing more and more from one another. It explains the existence of common characteristics in the varieties of rock belonging to a single center of eruption, and the absence of well-marked types of individual rocks. It accounts for the extremely local character of groups of rocks connected with particular centers of eruption, and at the same time their broader relations as members of the larger group of igneous rocks of the whole region. In this way it explains the local irregularities and repetitions in the order of eruption of volcanic rocks, and in its widest application it explains the differences between the rocks of various geographical areas over the globe.

MINERAL COMPOSITION OF ROCKS.

Some of these results may be discussed with advantage at this time. The one which exhibits the independence of the oxide molecules from any fixed combination, while in a

molten state, is the variability of the molecular proportions of the oxides in various rocks, and the consequent mineral composition of rocks.

A consideration of the chemical composition of igneous rocks shows no fixed ratio between the oxides which are treated as bases and the silica with which they combine to form most of the rock-making minerals. With the exception of iron, which is sometimes protoxide and sometimes sesquioxide, all of the elements appear in one state of oxidation, which seems to be fixed; but these oxides may be combined with silica in a number of stoichiometric proportions.

While there appear to be limits beyond which differentiation does not pass, and the result in any case is a silicate rock, still the variation of the elementary constituents may be very great, and it is possible that greater extremes than those already recognized may yet be found.

The dependence of the mineral development of rocks on the molecular proportions of the constituent oxides will be appreciated if we compare the theoretical molecular ratios of the commoner rock-making minerals with the chemical composition of the rocks in which they occur.

Molecular Proportions of the Rock-Making Minerals.

Orthoclase......	$K_2O : Al_2O_3 : SiO_2 = 1 : 1 : 6$
Albite..........	$Na_2O : Al_2O_3 : SiO_2 = 1 : 1 : 6$
Leucite.........	$K_2O : Al_2O_3 : SiO_2 = 1 : 1 : 4$
Analcite........	$Na_2O : Al_2O_3 : SiO_2 + 2\ aq = 1 : 1 : 4 + 2\ aq$
Nepheline......	$Na_2O : Al_2O_3 : SiO_2 = 1 : 1 : 2$
Sodalite........	$Na_2O : Al_2O_3 : SiO_2 + Na\ Cl = 1 : 1 : 2 + Na\ Cl$
Anorthite......	$CaO : Al_2O_3 : SiO_2 = 1 : 1 : 2$
Biotite........	$\begin{cases} (K, H)_2\ O : (Al, Fe)_2\ O_3 : SiO_2 = 3 : 3 : 6 = 3\ (1 : 1 : 2) \\ (Mg, Fe)\ O : SiO_2 = 12 : 6 = 6\ (2 : 1) \end{cases}$
Olivine.........	$(Mg, Fe)\ O : SiO_2 = 2 : 1$
Hypersthene...	$(Fe, Mg)\ O : SiO_2 = 1 : 1$
Augite........	$\begin{cases} CaO : (Mg, Fe)\ O : SiO_2 = 1 : 1 : 2 \\ (Mg, Fe)\ O : (Fe, Al)_2\ O_3 : SiO_2 = 1 : 1 : 1 \end{cases}$
Hornblende...	$\begin{cases} CaO : (Mg, Fe)\ O : SiO_2 = 1 : 3 : 4 \\ (Mg, Fe)O : (Fe, Al)_2O_3 : SiO_2 = 1 : 1 : 1 \end{cases}$
Riebeckite......	$Na_2O : Fe_2O_3 : SiO_2 = 1 : 1 : 4$

Acmite.........	$Na_2O : Fe_2O_3 : SiO_2 = 1 : 1 : 4$
Melilite.........	$(Ca, Fe, Mg) O : Al_2O_3 : SiO_2 = 12 : 2 : 9$
Sphene.........	$CaO : TiO_2 : SiO_2 = 1 : 1 : 1$
Perofskite	$CaO : TiO_2 = 1 : 1$
Ilmenite........	$FeO : Tio_2 = 1 : 1$
Magnetite.......	$FeO : Fe_2O_3 = 1 : 1$
Picotite	$(Mg, Fe)O : (Al, Cr)_2O_3 = 1 : 1$
Zircon..........	$ZrO_2 : SiO_2 = 1 : 1$
Apatite.........	$CaO : P_2O_5 = 3 : 1 + (Ca, Fl, Cl.)$

The table of molecular ratios just given shows that in the feldspars and feldspathic minerals the ratio of alkalies and lime to alumina is 1 : 1 in every case, and that the silica is variable; hence the controlling factor in the production of any one of the alkali feldspathic minerals is the amount of silica available. It is not the presence of large amounts of alkali in rocks that causes the crystallization of leucite and nepheline, though these minerals usually occur in very alkaline rocks, but it is the relatively low percentage of silica, for leucite occurs in rocks in the Yellowstone Park region which contain but little alkalies, the silica being insufficient to form orthoclase. For the same reason the feldspathic minerals low in silica: leucite, nepheline, sodalite, and analcite, do not occur in rocks in which quartz is an essential ingredient. A similar relation between the amount of silica and the development of olivine or hypersthene is noticeable, the essential difference between the two being the ratio of silica to the protoxides.

The chemically intermediate position of biotite between feldspar and olivine is important. It consists of a molecule with the form of that of feldspar, except that it would be a potash nepheline with the ratio of $K_2O : Al_2O_3 : SiO_2 = 1 : 1 : 2$, in which part of the potash is replaced by hydroxyl, and part of the alumina by ferric oxide. With this is combined an olivine molecule, $(Mg, Fe) O : SiO_2 = 2 : 1$. The development of biotite, under certain conditions, in place of olivine is discussed in connection with the granular rocks of Electric peak and with that of Crandall basin. Biotite should be expected in rocks with potash in which the alumina is

somewhat below the average and in which silica is low. It is well known to occur in those high in silica and potash, with but little iron and magnesia, being the chief ferromagnesian mineral of many rhyolites. In magmas rich in titanic acid it is evident that with sufficient silica sphene may be formed, but with insufficient silica perofskite will take its place. It is also evident that it is in rocks low in silica and alumina and rich in lime, iron, and magnesia that melilite is to be found; while in rocks low in silica and high in alumina picotite occurs.

The proportion of alumina in rocks affects the combination of the alkalies and lime. If it is not sufficient to combine with these in feldspar molecules the alkalies enter into combination with ferric oxide in an acmite molecule and perhaps a riebeckite molecule which has the same ratio, but takes the form of amphibole, and the lime must enter into a pyroxene or amphibole molecule or, in rare cases, into melilite. An excess of alumina increases the aluminous members of the pyroxene and amphibole molecule.

It is evident from the study of the molecular proportions of the essential oxides of various rocks and of their constituent minerals that by the differentiation of a magma of intermediate composition varieties of magma are produced in some of which silica is in excess of that required to form silicate compounds with the other constituents, and as a result more or less abundant quartz crystallizes out, while in other varieties the silica is insufficient to combine with all of the other constituents, and the least silicious compounds are formed together with those without silica, such as picotite and perofskite. This also proves the absence of fixed silicates in the molten magma.

ROCKS OF EXTREME DIFFERENTIATION.

The continued differentiation of an extremely localized magma leads to the production of an intimately related series of rocks which have been successively erupted at one center of volcanic activity, such as Electric peak and Cran-

dall basin, the extreme forms differing widely from one another. These extremes or end results of the differentiation of a particular body of magma are usually of smaller volume than the earlier eruptions at that center, and also constitute less common rocks, since differentiation has not advanced to the same extent in all localities. Moreover, it is to be expected that the end products of volcanic activity in a particular locality, which are of small volume, will appear as the last eruptions at the surface, or may in some cases never reach the outside of the volcano. In the first instance they will be among the earliest rocks to suffer from erosion, and, being of small volume, will soonest disappear, leaving only occasional remnants of their former existence. The portions of them which remain beneath the surface and are exposed by erosion may occupy relatively small spaces among the earlier rocks and will most frequently take the form of dikes, though it is not to be imagined that this is the only form in which they may occur. It is to these exceptional varieties of rock that Rosenbusch has applied the term "ganggesteine" or "dike-rocks," because, as he states, they are not found in any other mode of occurrence.*

All other rocks in certain places constitute dikes; but since they also occur in other ways they are not included in the particular group of "ganggesteine." It follows, therefore, that if the end results of differentiation are found in other modes of occurrence than as dikes they cease to belong to the group just named.

The application of the term "ganggesteine" to a very small number of all of the rocks known to occur in dikes appears to the writer to have been unfortunate, since it is not at all distinctive, its sole claim resting on our ignorance of the complete occurrence of the rocks to which it has been applied. It is consequently, as Rosenbusch himself realizes, in the nature of a vanishing term, whose rate of disappearance is proportional to our advance of knowledge.

Rosenbusch (H.) Mikroskopische Physiographie der Massigen Gesteine. 8°. Stuttgart, 1887, p. 277.

Exceptional dikes and flows in the Absaroka range.—In the region of Crandall basin and along the eastern portion of the Absaroka range there are dikes of rocks whose mineral composition shows them to be extreme members of the series of varieties belonging to several centers of volcanic activity.

They range from rocks rich in olivine and augite, with a groundmass of alkali feldspars and ferromagnesian silicates, to those with abundant alkali feldspar and mica. Among the first kind is one with a groundmass of orthoclase and leucite; another with a less crystalline groundmass of alkali feldspar, augite, mica, and magnetite. These grade into olivine-bearing augite-minettes, kersantites, camptonites and syenite-porphyry or trachyte. They form a series grading into the normal basalts of the region and appear from their mode of occurrence to be among the last eruptions of the centers with which they are connected.

Their chemical composition, which is given in Table III, analyses Nos. 1, 2, 3, 4, 6, 7, 9, 10, 11, 13, 16, 18, 19, and diagrams 3, 3*a*, 3*b*, shows that they are the result of the chemical differentiation of a phase of the basaltic magma. They represent the tendency of this magma to separate in magmas rich in magnesia, lime, and iron and low in alumina and relatively low in alkalies, and into others low in magnesia, lime, and iron and high in alumina and alkalies. It is to be remembered that it is characteristic of this region that the alkalies are higher for the whole group than for those of the Great Basin and some other localities.

By their mineral composition and microstructure, as well as by their chemical composition, these exceptional varieties of dike rocks exhibit the closest resemblance to the most typical of Rosenbusch's "ganggesteine," namely, the lamprophyres. They constitute syenitic and dioritic lamprophyres and syenite-porphyries, and prove most conclusively the correctness of Rosenbusch's statement that these kinds of rocks have resulted from the separation or differentia-

tion of the general magma with which they are associated.* But in the lava flows which were erupted near the end of the volcanic action which produced these extreme varieties of rocks and which lie immediately beneath the ejectamenta of a later series of eruptions, there occur varieties which correspond to the extreme forms of the dike rocks. The correspondence is partly in chemical composition and partly in mineral composition and habit, and also in association.

There are three localities in which the surface equivalents of these exceptional rocks have been observed. One is near the Crandall and Stinking Water centers, on the high peaks between Lamar river and a northwest branch of Stinking Water river. The surface flows are basaltic in character, carrying phenocrysts of olivine and augite and sometimes plagioclase. In one instance the groundmass consists of alkali feldspars with leucite, and the chemical composition of the rock, analysis 12, Table III, shows its similarity to other flows to be mentioned, and to the minette, analysis 7. Associated with and overlying these alkali basalts is a small fragment of sanidine-mica-rock, which is the equivalent of the syenite-porphyry or oligoclase-porphyrite of the dikes. Other orthoclase basalts form flows farther west and have similar chemical compositions, analyses 5 and 8.

A second occurrence is on a branch of Beaverdam creek, a short distance southeast of the Yellowstone lake. Here there are two sheets of alkali basalt, one of which is a leucite-orthoclase-basalt or basanite, and the other an alkali-plagioclase-basalt. The chemical composition of these rocks is shown by analyses 14 and 15. The leucite rock is the younger of the two. This rock, analysis 14, has nearly the same composition as the minettes, whose analyses are Nos. 7 and 10. The second rock, an alkali-plagioclase-basalt, analysis 15, is closely related in composition to the orthoclase-basalt-flows, but corresponds more nearly to the mica-augite-andesite, analysis 16.

* Rosenbusch (H.) Mikro. Phys. d. Mas. Gest. 8°. Stuttgart, 1886, vol. II, p. ix; also "Ueber Monchiquite," etc. Min. u. petr. Mitth. 8°. Vienna, 1890, vol. 11, p.31.

Immediately over these basalts is a sanidine-mica-tuff like the pebble found at the locality farther north. The great body of volcanic rocks in which these exceptional varieties occur is normal andesite of various kinds, in which no crystals of sanidine have been developed.

A third occurrence of alkali basalts is at Two Ocean pass, 18 miles south of Yellowstone lake. There are five sheets of basalt of different habits; two are characterized by abundant microscopic sanidine in the groundmass, besides some analcite. One has abundant phenocrysts of olivine and augite, and the other has few phenocrysts of these minerals and some of oligoclase. The chemical composition of the orthoclase-basalt, analysis 17, shows it to be similar to the leucite basalts just mentioned, and also to the minettes, analyses 7, 10, 18, two of which, however, are lower in silica, and the other somewhat higher in alkalies.

One of the basalt flows is rich in phenocrysts of augite and olivine, with none of feldspar, and if analyzed would undoubtedly be found to be high in magnesia and lime and low in alumina, which would correspond to the camptonitic varieties of the dike rocks.

These basalts are associated with the same kind of sanidine-mica-tuff that occurs on Beaverdam creek. In both cases this tuff is so filled with fragments of basalt and andesite that it is not possible to determine its chemical composition.

In all these cases the closely associated alkali basalts and sanidine rocks are the extreme forms of differentiation of the general magma and are the surface equivalents of certain varieties of the olivine-augite-minettes, kersantites, and syenite-porphyry or trachyte. The phenocrysts are alike in the corresponding forms of the different varieties, but the groundmass has crystallized differently.

A surface flow of sanidine lava occurs in the andesitic and basaltic breccias and flows immediately north of the Park. It is glassy and approaches rhyolite in microstructure and composition, but carries no quartz phenocrysts. The por-

phyritical crystals are sanidine and plagioclase, with a little augite, biotite, and magnetite. Its occurrence as a contemporaneous flow in the midst of great accumulations of basalt and andesite, as well as its chemical composition, shows that it may be considered as a product of extreme differentiation. Its chemical analysis has been placed in Table III, and is number 20. In silica percentage it corresponds to some dacites, but it is lower in lime and magnesia and much higher in potash. It forms a connecting link between trachyte and rhyolite.

"Ganggesteine" in other parts of the world.—Let us consider briefly the general character of the rocks which Rosenbusch has grouped together as "ganggesteine." They are classed under three sub-groups: Granitic, granite-porphyritic, and lamprophyric.

The first includes aplite and pegmatite.

The second embraces granite-porphyry, syenite-porphyry, eleolite-syenite-porphyry, and diorite-porphyrite.

The third includes the less common kinds of rock called lamprophyres, whose chief characteristic is the preponderence of phenocrysts of the ferromagnesian minerals and the scarcity or absence of those of feldspar and quartz. According to the feldspathic component of the groundmass, they are classed as syenitic lamprophyres and dioritic lamprophyres. To the first belong minette and vogesite, and to the second kersantite and camptonite.

These rocks have been called "ganggesteine" because they are said to attain their most typical development in dikes, though they sometimes appear as facies of large granular masses. They are said not to occur on the surface of the earth as extrusive bodies. In exactly what sense this is to be taken is not quite clear. It is, of course, understood that in the case of the highly crystalline kinds there are no extrusive equivalents that have the same crystalline structure; but their exact chemical equivalents are of frequent occurrence, although Rosenbusch states that he finds the extrusive equivalents of the granular rocks differ from them to

some extent chemically.* The incorrectness of this idea has been discussed by the writer in the paper on Electric peak.†

It is evident in the case of the granular core at Crandall basin that the aplite dike, analysis 16, Table II, is similar in chemical composition to certain varieties of the rhyolite of the Yellowstone region, which form immense bodies on the surface of the earth, and may therefore exist in some place within the earth in large bodies where the crystallization may attain a coarse grain.

The same is undoubtedly true for other varieties of the granite-porphyritic "ganggesteine." In fact, equivalents of a number of them may be found among the rocks classed as "volcanic" by Rosenbusch.‡ Their chemical equivalents occur as extrusive rocks and the differences between them are due to their mode of crystallization.

If we seek the extrusive equivalents of the lamprophyric rocks we shall first have to discover their chemical characteristics. They are often considerably altered and carry more or less calcite, so that chemical analysis fails to show the original composition of the unaltered rock. Moreover, many analyses have been poorly executed and are misleading. It is not to be expected, therefore, that a study of all of the analyses of lamprophyric rocks which have been collected by Roth in his tables will lead to very definite conclusions. One is somewhat surprised, nevertheless, to observe the wide range in composition which obtains for rocks which have been called minette and kersantite.

Table VII shows the range in percentage of silica for each

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* Rosenbusch (H.) Mikro. Phys. d. Mas. Gest. 8°. Stuttgart, 1887, p. 338; also "Ueber die chemischen Beziehungen der Eruptivgesteine." Min. u. Petr. Mitth. 8°. Vienna, 1889, vol. 11, p. 147.

† Iddings (J. P.) The Eruptive Rocks of Electric Peak and Sepulchre Mountain, Yellowstone National Park. Twelfth Annual Report of the U. S. Geological Survey. 8°. Washington, 1892, pp. 656, 657.

‡ Rosenbusch (H.) Mikro. Phys. d. mas. gest. 8°. Stuttgart, 1886; p. 380, microgranite and granite-porphyry; p. 456, diorite-porphyrite and quartz-diorite-porphyrite.

of these kinds of rock. Minette varies from 41 to 67 per cent., and kersantite from 45 to 63 per cent., which corresponds to a range of silica from ultra-basic basalt to dacite, or nearly the limits of rock variation. The great majority of them, however, are below 60 per cent. While very little value can be attached to most of these analyses, still some general idea of the composition of the rocks can be obtained.

A number of analyses of minettes have been copied into Table VIII, and another group of those of camptonites, monchiquites,* and kersantites have been arranged in Table IX. With them have been placed two analyses of allied rocks, which have been described by Dr. J. Francis Williams,† whose early death has proved a serious loss to the ranks of American geologists.

The great irregularities that exist in these series of analyses are partly due to the altered condition of the rocks, partly to the wide range of rock varieties which have been classified under the heads of lamprophyres, and partly to inexact methods of analysis. With all these sources of error, it is nevertheless apparent that as a whole the minettes are characterized by a low percentage of alumina and high magnesia, and that the alkalies are relatively high, with potash generally in excess of the soda. The analyses of kersantite are scarcely distinguishable from those of minette. They are characterized by a low percentage of alumina and high magnesia. The alkalies are not quite as high as in the minettes, and the potash is usually greater than the soda, but several of the analyses in both groups are alike. The few complete analyses of camptonites, the typical rocks having been described by Hawes, show it to be an extremely basic rock, moderately low in alumina, high in ferrous iron, and moderately high in lime and magnesia. The alkalies

* Rosenbusch (H.) and Hunter (M.) Ueber Monchiquite, ein camptonitisches Ganggesteine aus der Gefolgschaft der Eläolithsyenite. Min. u. petr. Mitth. 8°. Vienna, 1890, vol. 11, p. 446.

† Williams (J. F.) Annual Report of the Geological Survey of Arkansas for 1890. 8°. Little Rock. Vol. 2, p. 107. The Igneous Rocks of Arkansas.

vary considerably, and the rocks are somewhat altered, and contain more or less calcite. In the closely related monchiquites and fourchite there is a considerable variation in the alumina and alkalies, the iron and lime being high and the magnesia low. Fourchite is extremely high in potash, and in ferric oxide and lime.

Whatever may be thought of the chemical uncertainties of these lamprophyric rocks, it is possible to find chemically equivalent rocks among the extrusive lavas. A series of analyses has been selected from Roth's tables for comparison with those just given. They form Tables X and XI. The first includes leucite- and nepheline-basalt from the Eifel; basalt and dolerite from Meissner and Tuscany; mica-melaphyre from the Hartz; mica-basalt from Arizona, and andesite from Mytilene. With one or two exceptions, these rocks are low in alumina and high in magnesia, lime, and iron. The alkalies are somewhat variable, but in general the potash is in excess of the soda. In the mica-melaphyres the lime is low and the potash high. The excess of potash is particularly noticeable in the mica-basalts from Arizona.

The second group includes leucite and nepheline rocks and hornblende-basalts, with dolerite, augite-andesite, and trachyte; also feldspar-basalt and limburgite from Cape Verd islands. The last two exhibit a close resemblance to the camptonites. In general the alumina is low, the iron, lime, and magnesia are high, and the soda exceeds the potash, though in several analyses the potash is in excess.

A study of these analyses and others which might be produced makes it clear that rocks with the same chemical composition as the lamprophyres occur upon the surface of the earth as lava flows. It must therefore be the different crystalline development and mineralogical character assumed by these magmas in different modes of occurrence which distinguishes them from one another. This is only true to a certain extent, for it is evident from the mineralogical character of some of the lavas mentioned that they

are closely related in mineral composition to the dike rocks in question. Examples of these are the hornblende-basalt, mica-melaphyres, and mica-basalts, besides rocks which Rosenbusch has described as having camptonitic and minette-like characters.*

There are some, however, in which the mineralogical difference is essential, the one group being characterized by abundant mica, the other being free from it, and having certain other distinguishing features; but this distinction does not depend simply on the occurrence of the magma in dikes, for it is well known that dikes of all of the volcanic lavas may be found, the rocks being almost, if not absolutely, identical in both cases; consequently magmas that are chemically the same as those of the so-called "ganggesteine" occur in dikes, but have crystallized into rocks which differ from "ganggesteine" to as great an extent as any of the surface flows of the same magmas; hence it is not the occurrence of these rocks in dikes which has rendered them unusual, but it is the existences of certain physical conditions at the time of their crystallization which led to the development of particular mineral associations. In the great majority of cases the lamprophyres are characterized by the presence of biotite, sometimes with orthoclase, at others with plagioclase, and occasionally with olivine. The extrusive equivalents are generally rich in olivine, with no biotite, and in many cases carry leucite and nepheline or sanidine and alkali plagioclase. From the nature of the theoretical molecule of biotite, which is $(K, H)_2 O, (Al, Fe)_2 O_3, 2 SiO_2 + 12 (Mg. Fe) O, 6 SiO_2$, it is evident that if the silica of a magma is too low to convert all the alkali-alumina molecules into silicates, with the ratio 1 : 1 : 6, and the magma is rich in magnesia and potash and low in alumina, it may split up into olivine crystals, n $[2 (Mg, Fe) O, SiO_2]$, and leucite crystals, m $[K_2O, Al_2O_3, 4 SiO_2]$, besides an

* Nepheline rocks with camptonitic habit (Mikro. Phys. d. mas. gest. 8°. Stuttgart, 1887, vol. 2, p. 795). Hornblende-basalts (ibid., p. 738). Mica-trachyte of Monte Catini, Tuscany, an equivalent of minette (ibid., p. 597).

excess of iron in the form of magnetite and the lime-bearing constituents, or it may separate into biotite, x [12 (Mg, Fe) O, 6 SiO_2+$(K, H)_2$ O, $(Al, Fe)_2$ O_3, 2 SiO_2], and orthoclase, y [K_2O, Al_2O_3, 6 SiO_2] plus the lime-bearing constituents, with or without olivine, which may be present in small amounts. This interchange of molecules has undoubtedly taken place in the extremely differentiated magmas which formed the dikes and flows in the Yellowstone Park region.

A similar development of biotite has taken place in the more crystalline forms of all of the other phases of the magma in Crandall basin and at Electric peak. Its development in the less crystalline lamprophyric varities is due to the higher percentage of potash and somewhat lower percentage of alumina. It is, nevertheless, in these rocks also a function of the degree of crystallization as well as of the chemical composition of the magma; consequently the characteristic features of lamprophyric rocks are the result of the extreme differentiation of a more normal magma, and of the physical conditions attending their solidification or crystallization. They are not dependent on their occurrence as dikes; hence the application to them of the term "ganggesteine" appears to the writer to be inappropriate.

These more or less exceptional rocks have the same origin as all of the other varieties of igneous rocks connected with one center of volcanic activity—they are the results of the chemical differentiation of a general magma. They differ from the commoner varieties by being extreme forms of the differentiation. Like them they occur on the surface of the earth as extrusive masses, or within the crust as intrusive bodies, and in each case they assume different mineralogical and crystalline characters, according to the physical conditions attending their solidification. The physical conditions influencing crystallization, as already shown by the writer in another place, differ widely in geologically similar occurrences, but more especially in dike-like bodies, where the temperature of the surrounding rocks is the most essential factor; consequently a classification of

igneous rocks, if it is to be based on genetic relationships, cannot be founded on the geological mode of occurrence of such rocks, either as surface flows, or dikes and intrusive sheets, or laccolites and batholites, or as deeply-seated or superficial masses.

LOCALIZATION OF VOLCANIC PHENOMENA.

Nothing is more striking upon studying a great region of volcanoes or of volcanic rocks than the local character of volcanic phenomena. It has been specially commented on by King in the discussion of the origin of volcanic species, already cited in the historical part of this paper. Active volcanoes within the same district not only erupt independently of one another, but produce lavas of widely different composition. They are thus immediately connected with different kinds of magmas. A region of extinct and dissected volcanoes, such as those on the outskirts of the Yellowstone Park, furnishes a still better idea of the localized character of volcanic activity, because the complete series of events which produced at each center a great variety of rocks can be observed, instead of the external phenomena at a particular period in the history of a group of active volcanoes.

Irregular order of succession.—The investigation of such a region shows that at each center of volcanic action or at each volcano there has been erupted a series of different kinds of rocks, which followed one another in a definite order, with slight exceptions; and, further, that at various centers the range of the series of rocks has been different in extent and also different in kind; one group being mainly basic magmas, another mostly acidic, and so on. Moreover, the series at the various centers may overlap one another in the range of their variations, producing a recurrence of varieties; and since neighboring centers may be erupting different phases of the rock series at one and the same time, as we learn from active volcanoes, where the ejectamenta of different volcanoes commingle, the same kinds of rock may occur in different parts of the whole complex series representing the order of

eruption of the rocks in one region. In other words, there may be a recurrence of partial series of rock varieties in the whole series.

It is also possible to find a recurrence of different varieties at one center of eruption, which may be accounted for by supposing successive supplies of magma from some depth, which differentiate into similar varieties before their final eruption. This is particularly noticeable in a region of neighboring volcanic vents, such as the Yellowstone Park, and is in contrast to the infrequent recurrence of different kinds of rocks in a series of fissure eruptions such as may be found throughout the region of the Great Basin of western America.

There is often a great difference in the size of bodies of similar kinds of rocks which belong to the series which have been developed at various centers or belong to fissure eruptions.

It has already been remarked that the localized character of certain groups of rock varieties, together with their broader connection as members of the whole group of one region, and the consequent recurrence of similar rocks in the series representing the order of succession in point of time are explained by the theory of the chemical differentiation of a general magma in consequence of differences of temperature in different parts of it. Let us consider how such a process may have taken place.

Physical considerations.—The theory of differentiation presupposes a homogeneous fluid magma; homogeneous, because of the uniformity of the results in various regions, and fluid, because the evidence of volcanic lavas as well as the phenomena of crystallization * indicate that before crystallization commenced the magmas were fluid, whatever may have been their previous condition. Moreover, the differentiation of magma on a large scale has led to the same results as the differentiation of a small portion of magma which is known

* Iddings (J. P.) On the crystallization of igneous rocks. Bull. Phil. Soc., Washington. 8°. Washington, 1892, vol. 11, pp. 65-113.

to have been fluid, as in the case of a dike whose walls have a different composition from the middle; hence it is probable that magmas were fluid when chemical differentiation took place.

Since it has been assumed that the differentiation is due to differences of temperature in various parts of the magma, it follows that the magma must be located where it will experience differences of temperature; but in widely remote periods of time similar series of rocks have come to the surface of the earth; hence there must be a source of rock magma so situated that it has not been affected by differentiation during ages of the earth's history. In other words, the general magma from which a series of differentiations originated in Tertiary times must have been similar in composition to that from which a like series originated in pre-Cambrian time. Consequently the undifferentiated magma must have remained homogeneous through long lapses of time and must either have been situated beyond the zone of variable temperature—that is, in the region of constant temperature—or it must be solid and not susceptible to differentiation. It is necessary, therefore, to assume a stable magma, which at various periods of the earth's history has passed into a condition of instability. It is in accord with the physical nature of rock magmas to assume that they may exist in a state of potential liquidity, though in fact rigid—an excessive pressure overcoming the effect of high temperature.

Leaving the question of the condition of the interior of the earth to the physicists, we may start our speculations with a magma which has passed from a stable into an unstable condition.

The geological history of regions of volcanic activity shows that the volcanic phenomena have followed and also accompanied great orographic movements and profound dislocations of the earth's crust, and that molten magmas have risen to the surface through fissures. It also shows that differentiation has commenced with the earliest eruptions and con-

tinued to the close of volcanic activity. Hence the chemical instability of the original magma is coëval with volcanic action and succeeds orographic movements; from which we may conclude that the instability of the magma is a result of changes in the position of the earth's crust, whose greatest effect must be on the pressure of the underlying portion of the globe; so that the stability of the original magma must be a function of the pressure under which it exists.

Fissures as reservoirs of magma.—Fissures of vast dimensions must have penetrated to the seat of molten magmas in order to permit the escape of the enormous volumes of lava which form some massive eruptions in western America. It is self-evident that fissures, through which lavas rise to the surface, must open downward—that is, they must be in the nature of synclinal cracking, and will be large at the bottom and narrow at the top. They will undoubtedly be very irregular in shape, and often branched.

A great complex system of fissures having been formed, and having been filled with molten magma, it is evident that the magma will experience very different conditions in different parts of the system. In places it may reach the surface at once, and then be shut in. At other places a constant connection with the surface may be established, while in other directions it may not reach the surface, or may reach it at different times, as subsequent movements extend the fissures. The temperature in various parts of so irregular a reservoir would vary greatly because of the variable size of the bodies of magma and of the various rates of cooling due to the differences of conductivity in different kinds of enclosing rock. The differentiation of the magma will therefore progress at various rates and to different extents in different places.

Dynamical considerations.—It is to be remarked that beyond the fact of a chemical differentiation of rock magmas and the probability that it is the result of variations in temperature in different parts of the magma, nothing is known of its rate of action nor of the laws affecting a com-

plex solution. It is a chemico-physical problem involving differences of temperature, pressure, and all of the intricate properties of mixed solutions.

A comparison of the phenomena connected with a definite volcano or group of volcanoes and those of a region of fissure eruptions shows that at a volcanic center differentiation takes place in a smaller body of magma and proceeds to greater extremes within a given time, and is independent of the differentiation in other centers; but the resulting products are similar when the general magmas at these centers were similar. It is also observed that the eruptions of a volcano are comparatively frequent or continuous, and that the volume of the magma erupted at any one time is comparatively small, while in a region of fissure eruptions the outbursts are few and are often separated by long lapses of time, and the volume of lava at each outburst is comparatively large. The composition of the magma erupted at successive phases of the activity is more uniform, and the differentiation has evidently taken place in a larger body of magma than in the case of a volcano. The localization of the magma, therefore, was not so restricted, and we may well imagine that the differentiation took place farther beneath the surface of the earth than in a volcano, the resulting products being the same in kind, but not always in degree.

It is perfectly evident that the course of dynamical events must have great influence on the character of the magma erupted from different centers or in different regions. The volume of magma erupted at one time and the constancy of the supply will affect the uniformity of its composition; consequently the variations in the character of the rocks at different centers of eruption and the order of their eruption will depend on the course of dynamical events in various parts of the region.

The objection has been raised to any theory of separation which would derive such extreme kinds of rock as rhyolite and basalt from one original magma, that we should expect to find transitional forms of rock associated with them. But

an eruption of lava on the surface of the earth cannot be supposed to exhaust the reservoir from which it was drawn. It must rather be the escape of a comparatively small part of its contents. Moreover, the intermediate forms of the magma have been erupted previous to that of the extremes, and extremes of differentiation will be located where extremes of temperature in the reservoir have been established, and will be drawn off through different vents.

It would be an awkward task to explain how it might happen that in the course of volcanic events all of the rhyolite in one region should be erupted at once and all of the basalt at once, and always in a definite order of succession; but in regions of oft-repeated eruptions it is found that these rocks alternate with one another to a greater or less extent, and that transitional forms exist in limited amounts; so that it is the general law only which remains, namely, that the order of eruption for any complete series of igneous rocks is from intermediate to more and more extreme varieties.

Localized magmas. — The geographical distribution of groups of rocks that possess different chemical characteristics has a most important bearing on the question of the source of igneous magmas and the condition of the interior of the earth.

The preponderance of rocks rich in alkalies throughout large areas of the earth's surface and their scarcity in other regions, which has been pointed out in an earlier part of this paper, indicates a difference in the general magmas from which the whole series of igneous rocks of various regions have been derived. The vast extent of these chemically diverse areas shows the profound and persistent character of the differences.

This may be illustrated in a general way by considering the distribution of two broadly defined groups of rocks, one of which we may call the alkali group and the other the sub-alkali group. In the first we would include natural groups, parts of which are rich in alkalies, especially those rocks low in silica; but some of the members of these groups

are poor in alkalies. In the sub-alkali group are included natural groups, the great majority of whose members are relatively poor in alkalies, while a few may be comparatively rich in alkalies. This grouping is selected merely for present purposes, and is not suggested as a possible basis of classification, since it is too indefinite.

Sub-alkali groups of rocks extend over a large part of the Great Basin of western America and the Pacific coast into Mexico and Central America, and it is probable that the rocks of the Andes, in South America, belong to the same group; but it is possible that this belt is not an uninterrupted one, and may be broken into smaller areas of somewhat different characters, for it is known that leucite rocks occur in Lower California.

Alkali groups occur along the eastern part of the Rocky mountains and are known in Montana, Wyoming, South Dakota, Colorado, and a variety of them is developed in Arizona. Rocks of the alkali group, notably eleolite-syenites and associated varieties, occur in the central and eastern United States and Canada, having been found in Arkansas, New Jersey, Massachusetts, and Maine, and in the vicinity of Montreal. They are specially characteristic of Brazil and the islands off the east coast of South America, and nepheline and leucite basalts have been found along the eastern part of the Andes, in Argentine Republic, and Paraguay.

In Europe the region of alkali rocks is better known than that of the sub-alkali group. The former extends from the Scandinavian peninsula, through Germany, Bohemia, Austria, and France to Portugal, and continues across the northwestern part of Africa to the islands along its western coast.

Sub-alkali groups are characteristic of Great Britain and Hungary, and probably extend for a considerable distance eastward and southeastward, including the Grecian archipelago and the Caucasus, extending into Persia.

Alkali rocks occur in Kerguelen island and in southeastern Africa and the islands to the eastward. They also occur in Java.

From the wide distribution of the rocks of the alkali groups it would appear that they are of more frequent occurrence than those of sub-alkali groups; but this arises from the fact that it is easy to recognize the former by the presence of certain characteristic minerals, whereas the latter are not so easily characterized, and it requires more complete information concerning the chemical composition of the whole group or a knowledge of the mineralogical character of the various rocks of the whole series to establish its identity.

Upon investigating the data already accumulated, which indicate the character and distribution of the various igneous rocks over the earth, it is evident how essential it is that ample, and if possible complete, collections should be made of all regions where such rocks occur, and that, besides a mineralogical examination, there should be a complete and accurate chemical analysis of the material.

In the absence of such complete information it is not possible to establish definite laws regarding the geographical distribution of chemically different magmas, but sufficient is known of them to warrant the conclusion that the igneous rocks in various parts of the globe form natural groups of varieties in each region, and that they have resulted from the chemical differentiation of different kinds of general magmas—that is, they do not originate immediately from a universal magma of homogeneous composition, or from a homogeneous liquid couche beneath the crust of the earth.

However, the recurrence in different parts of the earth's surface of the same varieties of general magma leads to the conclusion that they may be the results of a differentiation of a still more uniform magma, which may have been homogeneous throughout its whole extent and may represent an original liquid couche. There is at present nothing to indicate at what period in the earth's history this supposed primitive differentiation took place. Its extreme remoteness would be proven if it could be shown that the chemical character of general magmas which furnished groups of igneous rocks in various periods from pre-Cambrian to the

present had been the same, and that they have had the same geographical distribution. This seems to the writer to have been the case with the great succession of volcanic eruptions that have occurred in Great Britain. But the recurrence of chemically different groups may be susceptible of another interpretation, which has been pointed out to the writer by Mr. Clarence King, who considers that the chemical character of the undifferentiated localized magma will depend on its position in depth within the earth, and that the depth at which solid matter is converted into fluid matter will be controlled by the loading or unloading that takes place at the surface. This hypothesis may be tested by comparing the character of each general magma with the geological history of the region in which it occurs.

In either case the geographical distribution of chemically different groups of igneous rocks proves that they have originated immediately from localized magmas, and not from a liquid zone of homogeneous magma.

RÉSUMÉ.

In conclusion, it may be well to trace the development of ideas regarding the nature and origin of igneous rocks as they have been expressed by the investigators cited in the historical portion of this paper, and to point out in what respect the views of the present writer tend to modify them.

The conception of a differentiation of a primary homogeneous mass as the cause of the differences in volcanic lavas was expressed definitely by Scrope in 1825. To his mind the previously undifferentiated mass was a crystalline rock with the character of granite, and the process by which the separation was supposed to have taken place was described as an intumescence due to interstitial water being highly heated and dissolving part of the quartz in the rock, and which was accompanied by the volatilization of the mica and by the forcing out of the gaseous portion by pressure, and its concentration and crystallization elsewhere.

Nineteen years later, when Charles Darwin turned his attention to the problem of the origin of igneous rocks, the idea of the differentiation of a homogeneous mass took a somewhat different form. The undifferentiated mass became a molten magma, and a state of partial crystallization was considered the condition in which a separation of its constituents could take place by the force of gravity. He, however, added to this hypothesis another, which conceived the partially crystallized molten magma to be capable of draining off the more liquid portions into open fissures and abysses.

In contrast to the crudeness of these conceptions are the expressions of Dana, published in 1849, five years after those of Darwin. Dana appreciated the gradual transition in composition between the various kinds of rock forming the volcanoes of the Pacific islands, and also the transitions in crystalline texture from glassy to granular forms. He stated clearly his opinion that the crystalline character of all igneous rocks depended solely on the conditions of pressure, temperature, and slowness of cooling under which molten magmas had solidified. He did not attempt to account for the diversity in composition of igneous rocks in general, but confined his speculations to the differences which were supposed to exist within the body of a volcano. At that time it was considered that the central part of a volcano was usually composed of more feldspathic rock than the surface lava flows.

Although subsequent methods of research may have modified our knowledge of the petrological character of the mass of a volcano, the conception which Dana had of the process by which the material of lavas became differentiated is noteworthy. It was a differentiation which antedated crystallization and took place in a fluid molten magma. It was based on the assumption that as the magma began to cool the feldspathic constituents would grow more viscous than the pyroxenic, and the vaporizable ingredients would tend to inflate the mass as it approached the surface and would lower the specific gravity of the more liquid portion, which

would urge it upward more freely and thus cause a separation. The more important feature of his conception, however, is the idea that the components of a molten magma do not necessarily represent fixed mineral molecules, and that the magnesia and silica, and consequently the other oxides, are capable of entering various compound molecules, according to conditions of temperature.

A wholly novel theory of the origin of the variability of volcanic rocks was conceived by Bunsen and published two years after that of Dana. It was one involving the synthesis of two distinct magmas, and, as Sartorius von Waltershausen has remarked, was purely imaginary and unnatural. It emphasized the differences and not the resemblances of rocks, and has maintained a strong hold on the minds of those who have been most impressed with the distinguishing characteristics of volcanic rocks.

The idea of the essential nature of the differences among igneous rocks took a more rational and permanent shape in the theory of Von Waltershausen, who imagined the material of the earth to be arranged in zones of increasing specific gravity from the surface inward, and that the eruptive rocks varied in composition with the geological age of their eruption; so that the more silicious and specifically lighter ones were drawn from nearer the surface than the more basic rocks, and were erupted at a time when the crust of the earth was not as thick as it became subsequently by the gradual refrigeration of the globe.

The transitions in composition between different kinds of rock were due to the gradations of the material of the zones, which rested on the supposed primary arrangement or differentiation of the original fluid magma of the earth by a process of gravity separation. This is essentially different from Bunsen's synthetical theory, though it is frequently alluded to as a modification of the latter. It was based on a primary differentiation and has no element of synthesis in it. It was in accord with a widely accepted view of the order of eruption of igneous rocks, namely, that the compo-

sition and character of such rocks varied with the geological age in which they were erupted—a belief which has been retained with great persistency by a large body of geologists, although combated from the first by Scrope, Dana, and others.

From the time when Von Waltershausen enunciated his theory on to the present the various conceptions of the origin of volcanic rocks have sprung from one of these early sources, with one exception. They may be designated as the theory of local differentiation, that of synthesis, and that of primitive universal differentiation.

To the first class belong the views expressed by Lyell and Jukes. The former credits Scrope with a theory which appears to be a special form of Dana's hypothesis rather than an elaboration of Scrope's ideas of 1825, since he did not then conceive of a fused magma. Jukes introduced the idea of an accession of material from the rocks penetrated by the fused magma, which was previously suggested by Bunsen. He enlarged upon the supposed effect of the physical conditions attending consolidation, combated the assumed relationship between the character of rocks and the age of their eruption, and suggested that in place of the terms *ancient* and *modern*, as applied to igneous rocks, be substituted the expressions *deeply seated* and *superficially formed.*

The theory of Durocher, which followed that of Von Waltershausen by four years, resembles the latter's theory more closely than that of Bunsen, although it emphasizes the idea of two magmas, which corresponded to those of Bunsen; but these were based on an estimation of the supposed quantitative relations of igneous rocks, and not on an assumed synthetical origin for them. The silicious and basic magmas formed two principal, universal zones within the earth, and were arranged according to their specific gravities, with a smaller zone of transitional character between; hence this much of the theory resembled that of Von Waltershausen, except in regard to the relative proportions of the different zones, and in the still more important fact that the zones remained fluid and the material erupted did not vary with

the geological age in which it was erupted. Durocher also realized the local character of volcanic action and introduced the idea of a local differentiation of part of the magma by a process of liquation such as takes place upon the cooling of certain metallic alloys. He also considered that variations in composition may be affected by the accession of sea water to the molten magma. The independent action of neighboring volcanic vents suggested the idea that the vents do not connect directly with the general reservoir, but with branching fissures.

A special form of synthetical hypothesis was conceived by Von Cotta. It consisted of one molten magma, which was basic, and a solid silicious crust, through which the magma was erupted and which it dissolved in various proportions.

In 1868 Von Richthofen applied Von Waltershausen's theory to the occurrence of volcanic rocks in Tertiary times, and recognized the difficulty of its application to the occurrence of trachyte and rhyolite, and also to the localization of volcanic phenomena, but did not offer any satisfactory explanation of these anomalies.

Ten years afterwards King elaborated the theory of local differentiation, having discarded the theory of a universal fluid zone of different specific gravities as incompatible with the constant repetition of series of rocks of variable composition from pre-Cambrian time to the present. According to King's view, a localized body of magma was rendered molten or fluid by a relief of pressure due to erosion. After cooling and crystallization had commenced in the magma a partial separation of the material would take place by the force of gravity. The character of the lava erupted would depend on the phase of separation at which the eruption took place. This differentiation, however, did not account for all of the variations among the volcanic rocks of a region, but only for the modifications of each of the four principal groups which he considered had been satisfactorily established, namely, the propylitic, andesitic, trachytic, and neolitic, which latter embraced rhyolite and basalt. For each of

these groups, which had a quartzose, intermediate, and pyroxenic modification, it was necessary to assume a special ephemeral lake or reservoir, and therefore a special course of erosion. Moreover, the reservoirs were supposed to have been produced at successively greater depths within the crust of the earth, and since they differ from one another in composition, the material of the crust must vary in composition with the depth, and also in such a manner that zones of like composition must have been affected in the same regions at widely different times in order that andesites or rhyolites and basalts might be erupted at various geological periods.

Two years later Dutton advocated the theory (also held by others) that igneous rocks have been derived from the fusion of sedimentary rocks, and that their diversity is not due to a differentiation of a molten magma. This was based on the erroneous assumption that the original magma, of which the most ancient rocks were formed, must have had the composition of basalt, and that a differentiation of a molten magma must have been mechanical, and could not have taken place, but that the separation must have been brought about by processes of erosion and sedimentation. His speculation should be regarded more as a contribution to the physics of eruption than to the problem of the origin of volcanic rocks; for he admits his surprise that there should be as much regularity as there is to the order of succession of volcanic rocks upon his theory of their origin, and he also confesses that he knows of no cause for the increase of heat necessary for their production.

The next discussion of the origin of volcanic rocks is that by Rosenbusch, which, however, is confined entirely to their chemical characteristics. Upon purely chemical considerations, he is of the opinion that the variability in the composition of igneous rocks of all kinds must be the result of a differentiation of a generally uniform magma. Moreover, this separation is thought to have followed chemical affinities. By comparing a number of chemical analyses of dif-

ferent kinds of rocks he endeavors to prove the existence of certain definite, stoichiometrically proportioned molecules, which he terms "kerns." It is further assumed that these "kerns" are capable of dissolving any of the constituent oxides or metals in rocks to variable extents, which accounts for the impurities in the "kerns." The idea is also expressed that the differentiation of the general magma has produced the magmas which have solidified at some depths within the earth, while a secondary differentiation of these partial magmas has produced the rocks which have solidified at the surface of the earth. This is in accord with his opinion that extrusive rocks differ in chemical composition from their corresponding deeply seated forms. He has offered no suggestion as to the process by which a chemical differentiation could have taken place.

The idea expressed by Brögger regarding the origin of the igneous rocks of the region of Christiania is that, by some process not explained, a basin of molten magma came into existence and began to cool at its margin. The magma is considered to be a solution of silicates, and a diffusion of the less soluble silicates toward the cooler margin is thought to be the principal cause of its differentiation. He suggests that a partial crystallization may set in and lead to the production of segregations. A settling of these crystals to the bottom of the reservoir and their subsequent fusion would account for the final eruption of basic magmas. The distinctive chemical character of this natural group of rocks is pointed out.

Vogt's conception of the differentiation of a molten magma, even in extremely localized instances, is that it is due to molecular diffusion, without the actual separation (or crystallization) of minerals, and that it may be brought about by differences of temperature and of pressure, and possibly by magnetic attraction in the case of iron molecules.

Dakyn and Teall express the idea that in the region investigated by them the differentiation of a general magma took place during the process of consolidation, and that it

was a consequence of the formation of minerals in a definite order. Diffusion, according to Soret's principle, and the subsidence of crystals may assist in producing heterogeneity in the original magma.

It now remains to be pointed out in what manner and to how great an extent the researches and observations of the present writer tend to modify and, it is hoped, to advance the theories just reviewed.

The more detailed investigation of large areas of volcanic rocks fails to establish the individuality of the four principal groups suggested by King, and proves that all of the volcanic rocks of one region belong to a single group. In such a group there are gradations in composition from intermediate to extreme varieties, and rhyolite and basalt represent the extremes in many regions. This conclusion has also been reached by Hague in his work on the Eureka District.

The study of greatly eroded accumulations of volcanic rocks establishes the correctness of the conception definitely stated by Dana, that the crystalline character of all igneous rocks depends solely on the physical conditions under which molten magmas have consolidated, and therefore all of the igneous rocks which belong to the same period of volcanic activity, whether extrusive or intrusive, constitute a natural group.

The consanguinity of the various rocks of such a natural group is shown by their mineralogical, chemcial, and geological relationships; hence, instead of four separate sources for the volcanic rocks of a given district, we are led to the consideration of one common source for all of the igneous rocks belonging to an era of volcanic action in one region.

The fact of a differentiation of some common magma, and not the compounding of two or more different magmas, is established by the geological occurrence and order of eruption of the various members of a natural group of igneous rocks, and also by their petrological and chemical characteristics; but an investigation of the chemical variability of the

rocks of a natural group proves that the differentiation has not been in accordance with definite stoichiometric proportions, and that such definite molecules as those designated by Rosenbusch as *kerns* cannot be shown to exist in magmas and do not control the final molecular grouping of crystallization. It proves the absence of definite silicate molecules and the capability of the interchange or shifting of the elementary oxide molecules, both in the differentiation of magmas and in their crystallization into minerals. This negatives the conception of a partial crystallization of magmas and their differentiation by the settling of heavier minerals—an idea which is also disproven by evidences of the comparatively rapid and relatively recent growth of the porphyritical crystals in rocks and the crystallization *in situ* of the minerals of most coarsely granular rocks, and also by the uniform distribution of crystals of magnetite and augite in such rocks as rhyolites. The differentiation of molten magmas must therefore be of a chemico-physical nature.

Conceptions of the chemical and physical character and behavior of solutions are rapidly being developed. Much, however, remains to be learned of the true nature of the simplest aqueous solution. The complexities of a compound solution that exists only at extremely high temperatures and experiences the pressures to which rock magmas have undoubtedly been subjected may long remain beyond the reach of direct investigation. Still the steady advancement of experimental physics offers great possibilities in this direction. Until the establishment of definite knowledge concerning the nature of molten magmas we must proceed along the lines of analogy by applying to them such laws as may be found applicable to solutions that exist at lower temperatures and pressures.

The most recent theories regarding the nature of solutions conceive of a greater or less dissociation of the compounds dissolved, or their partial separation into ions, the extent of which dissociation depends on the degree of dilution—that is, on conditions of relative mass, temperature, and pressure.

There have been established certain laws of concentration, one set of which express the principles of osmotic pressure, while the other takes into account the force of gravity. The application of the first is found in the fact that in the cooler part of a solution there will be a concentration of material in solution, and this action is of a decided and comparatively energetic nature. The second principle is much less active, and requires marked differences of gravity and a long extent of time to take effect.

The localized character of the phenomena of volcanicity and of minor groups of igneous rocks proves the localization of differentiation. And the broader relationship of all the minor groups of rocks in one region establishes the existence of a more general differentiation, which has acted on a more extensive magma, from which all of the igneous rocks of the region have originated. This leads to the conception of a branching system of fissures in which the process of differentiation may take place with more or less localization—an idea expressed by Durocher.

The geographical distribution of chemically different groups of igneous rocks proves that in various parts of the earth the sources or reservoirs from which these groups of rocks were immediately derived contained homogeneous magmas, which differed from one another in composition; hence they did not constitute a universal zone or couche of homogeneous material, but were localized reservoirs of magma.

This is further proven by the recurrence in various parts of the world of similar associations of chemically different regional groups of rocks, which indicates that the reservoirs of general magma, which differed from one another chemically, were themselves the result of a more uniform magma, which must have been universal, and may have belonged to a homogeneous couche.

The period in which this primitive differentiation took place appears to have been geologically remote, for the incomplete evidence at hand seems to show that the igneous

rocks in any given region, which have been erupted in various geological epochs since Archæan or pre-Cambrian times, belong to chemically similar groups, and have been derived essentially from the same magmas; hence the establishment of the chemical character of these magmas must have antedated Cambrian time.

It has been suggested elsewhere that these differences of composition may be connected with differences in the dynamical processes which have led to their eruption—a speculation which calls for a more thorough knowledge of the geological history of the various regions in which such groups of rocks occur.

The study of the petrology of igneous rocks, as already pointed out, leads to conclusions regarding the physical condition of the interior of the earth which are important, because they are tests of the conclusions derived from purely physical considerations. They may be summed up as follows:

The phenomena of volcanic eruption and those of the crystallization of igneous rocks, as well as the phenomena of differentiation, indicate that the magmas from which igneous rocks were derived were molten fluids when these processes took place.

But the occurrence at various geological periods of similar series of differentiations which were coeval with the course of dynamical events during each period of volcanic activity indicates that the general or undifferentiated magma was solid during long lapses of time—that is, it existed in a state of potential liquidity.

The coördination of all of the characteristics of igneous rocks, together with a knowledge of the physical laws which have produced them, will furnish the most material evidence we may expect to obtain of the true nature of the interior of the earth.

The incompleteness of the evidence which has been presented in this paper is one of its most obvious features, and emphasizes the importance of more thorough investigation

into the geological occurrence and geographical distribution of igneous rocks, as well as more complete and systematic analysis of their chemical composition. Realizing the incompleteness of our knowledge of the chemical, mineralogical, and geological relationships of igneous rocks, the writer cannot assume to have done more than call attention to their importance and point out the direction in which the future development of the science of petrology should be made.

The confusion which has overtaken the classification of igneous rocks and the burden which is being heaped up by the present tendency to multiply its terminology by creating names for each modification of rock will find their remedy in a more logical conception of the true nature of the differences of rocks and in a better understanding of the relationship between all of the rocks of a natural geological and geographical group.

ROCK ANALYSES.

Rocks of Table I.

From Electric peak and Sepulchre mountain and the Yellowstone Park plateau. The first twenty-one have been published in the paper on this region already cited. The last three analyses have appeared in an article on Obsidian Cliff by the present writer, in the Seventh Annual Report of the U. S. Geological Survey.

1. Pyroxene-andesite.
2. Hornblende-andesite.
3. Pyroxene-mica-diorite.
4. Hornblende-pyroxene-andesite.
5. Pyroxene-andesite.
6. Pyroxene-porphyrite.
7. Pyroxene-mica-diorite.
8. Hornblende-pyroxene-andesite.
9. Pyroxene-mica-diorite.
10. Quartz-pyroxene-mica-diorite.
11. Hornblende-mica-andesite.
12. Quartz-mica-diorite.
13. Quartz-mica-diorite.
14. Hornblende-mica-andesite.
15. Quartz-mica-diorite.
16. Dacite.
17. Quartz-mica-diorite-porphyrite.
18. Quartz-mica-diorite.
19. Dacite.
20. Quartz-mica-diorite.
21. Quartz-mica-diorite-porphyrite.
22. Rhyolite. Upper Geyser basin.
23. Rhyolite. Tower creek.
24. Obsidian. East of Willow park.
25. Obsidian. Obsidian cliff.
26. Lithoidal rhyolite. Obsidian cliff.
27. Obsidian. Obsidian cliff.

Rocks of Table II.

From Crandall basin, Absaroka range. Mostly rocks forming a granular core and dikes cutting basaltic breccia and flows.

1. Gabbro-porphyrite (core).
2. Basalt (flow).
3. Basalt (dike).
4. Mica-pyroxene-porphyrite (core).

5. Gabbro (core).
6. Basalt-glass (breccia).
7. Gabbro (rich in mica) (core).
8. Gabbro (rich in mica) (core).
9. Dioritic facies of gabbro (core).
10. Poikilitic facies of gabbro (second core).
11. Augite-porphyrite (Sill).
12. Porphyrite (hornblende-mica-pyroxene) (dike).
13. Quartz-diorite-porphyrite (core).
14. Quartz-mica-diorite (core).
15. Quartz-mica-diorite-porphyrite (core).
16. Aplite (dike or vein in core).

Rocks of Table III.

Dikes and Surface Flows of Exceptional Character in the Absaroka Range.

1. Olivine-augite-orthoclase-leucite-rock (dike?).
2. Basalt (mica and alkali groundmass) (dike).
3. Basalt (" " ") (dike).
4. Hornblende-minette (?) (dike).
5. Basalt with alkali-feldspars (flow).
6. Camptonite (?) (dike).
7. Minette (dike).
8. Basalt with alkali-feldspars (flow).
9. Balsalt (?) (mica and alkali-feldspars) (dike).
10. Minette (dike).
11. Basalt (dike).
12. Leucite-basalt (flow).
13. Basalt (?) (mica and alkali-feldspars) (dike).
14. Leucite-basalt (flow).
15. Basalt (with alkali-plagioclase) (flow).
16. Mica-augite-andesite (dike).
17. Basalt (alkali-feldspars) (flow).
18. Minette (or trachyte) (dike).
19. Syenite-porphyry (or oligoclase-trachyte) (dike).
20. Trachyte (glassy), approaching rhyolite (flow).

Rocks of Table IV.

From Vesuvius, Rocca Monfina, and Ischia.

1 to 11. Leucitophyre. Vesuvius.
12. Trachyte. Ischia.
13. Trachyte. Rocca Monfina.
14. Glassy shell of a bomb of leucitophyre. Vesuvius.
15. Leucitophyre. Rocca Monfina.
16. Glassy portion of a bomb of leucitophyre. Vesuvius.
17 to 24. Trachyte. Ischia. (19, obsidian).

Rocks of Table V.

From Etna, Pantelleria, Ferdinandia.

1 to 6. Dolerite basalt. Etna.
7. Dolerite basalt. Ferdinandia.
8. Dolerite basalt. Pantelleria.
9 to 10. Dolerite basalt. Etna.
11. Dolerite basalt. Pantelleria.
12 to 20. Dolerite basalt. Etna.

Rocks of Table VI.

From Pantelleria.

1, 2, 3. Augite-andesite.
4 to 11. Pantellerite.

Rocks of Table VIII.

Minettes.

1. Minette, Vosges, west of Mönkalb, near Barr (R. T., 1879, xxvi).
2. " " Haut de Steige (R. T., 1879, xxvi).
3. " Bohemia, Przibram (R. T., 1879, xxvi).
4. " Vosges, Framont (R. T., 1879, xxvi).
5. " Bohemia, north of Prag (R. T., 1879, xlvi).
6. " Heidelberg, Eichelberg (R. T., 1873, xxvi).
7. Syenite dike, Vosges, between Welchbach and Rohrlach (R. T., 1879, xxiv).
8. Minette, Heidelberg, Leonhardskopfes (R. T., 1873, xxvi).
9. " Odenwald, Mittershausen (R. T., 1869, lxii).
10. " Odenwald, Oberlaudenbach (R. T., 1869, lxii).
11. " Odenwald, Kirschhäuser Thal (R. T., 1884, xxiv).
12. " northwestern England, south of Haygarth (R. T., 1884, xxiv).

Rocks of Table IX.

Camptonites, Monchiquites, and Kersantites.

1. Camptonite, Montreal, reservoir extension (Harrington).
2. " Campton, N. H. (Hawes).
3. " " "
4. " " "
5. Fourchite, Fourche Mountain, Ark. (Brackett and Noyes).
6. Amphibole monchiquite, Magnet Cove (W. A. Noyes).
7. Monchiquite, Rio do Ouro, Brazil (P. Jannash).
8. Kersantite, Fichtelgebirge, Fussgrund, near Gohrm (R. T., 1884, xxiv).
9. Monchiquite, Sao Paulo, Brazil (M. Hunter).

10. Kersantite, Fichtelgebirge, Marlesreuth (R. T., 1884, xxiv).
11. " " between Falkenstein and Steinbachmühle (R. T., 1884, xxiv).
12. " Vosges, Laveline (R. T., 1873, xxx).
13. " Hartz, Krone (R. T., 1884, xxvi).
14. " Nassau, Adolfseck (R. T., 1873, xxxii).
15. " Hartz, Gegenthal (R. T., 1884, xxvi).

Rocks of Table X.

Volcanic Rocks Corresponding to Minettes in Chemical Composition.

1. Leucite-basalt, Eifel, Gossberg, near Walsdorf (R. T., 1879, lx).
2. Nepheline-basalt, Eifel, Bongsberg, near Pelen (R. T., 1879, lx).
3. Feldspar-basalt, Meissner, Kitzkammer (R. T., 1879, lxxii).
4. Mica-melaphyre, Hartz, Friedeland, near Neustadt, u. H. (R. T., 1884, xlvii).
5. Dolerite, Meissner, north side (R. T., 1879, lxxii).
6. Mica-melaphyre, Hartz, Friedeland, near Neustadt, u. H. (R. T., 1884, xlvii).
7. Mica-melaphyre, Hartz, Lehnberg, near Neustadt, u. H. (R. T., 1884, xlvi).
8. Melaphyre, Hartz, Kahleberg bei Hasselfelde (R. T., 1873, xxx).
9. Dolerite, Tuscany, Radicofani (R. T., 1869, cxxx).
10. Mica-basalt, Arizona, analyzed by Dr. W. H. Melville.
11. Mica-basalt, " " " "
12. Mica-basalt, " " " "
13. Andesite, Mytilene, north coast (R. T., 1879, lxx).

Rocks of Table XI.

Volcanic Rocks Corresponding to Camptonites, Monchiquites, and Kersantites in Chemical Composition.

1. Hornblende-basalt, Rhone, Sparbrod (R. T., 1884, lxxiv).
2. Hornblende-basalt, Rhone, southwest of Gersfeld (R. T., 1884, lxxiv).
3. Nephelinite, Eifel, Dockweiler (R. T., 1869, cvi).
4. Feldspar-basalt, Cape Verde, Santiago (R. T., 1884, lxxiv).
5. Limburgite, Cape Verde, Santiago (R. T., 1884, lxxxii).
6. Leucite-basalt, Eifel, Gossberg, near Walsdorf (R. T., 1879, lx).
7. Leucitophyre, Vesuvius, Granatello, 1631 (R. T., 1884, lvi).
8. Augite-andesite, southwest of Tiflis (R. T., 1879, lxx).
9. Dolerite, Steinheim (R. T., 1869, cxxxii).
10. Trachyte, Mont Dore, Rigolet Haut (R. T., 1873, xxxvi).
11. Dolerite, Euganäen, Teolo (R. T., 1869, cxxviii).
12. Dolerite, Meissner, West Plateau, Braushohl (R. T., 1869, cxxx).

TABLE I.

	Analyzed by—
Analyses Nos. 1, 2, 3, 5, 6, 7, 11, 12, 13, 15, 16, 17, 18, 20, 21, 24, 25, 26, 27	Mr. J. E. Whitfield.
Nos. 4, 8, 14	Dr. T. M. Chatard.
Nos. 9, 10	Dr. W. H. Melville.
No. 19	Mr. L. G. Eakins.
Nos. 22, 23	Dr. F. A. Gooch.

TABLE II.

Analyses Nos. 1, 2, 3, 4, 5, 6, 7, 8, 9, 11, 12, 16	Mr. L. G. Eakins.
Nos. 10, 13, 14, 15	Dr. W. H. Melville.

TABLE III.

Analyses Nos. 2, 3, 4, 5, 7, 8, 9, 12, 13, 14, 15, 16, 20	Mr. L. G. Eakins.
Nos. 1, 17	Mr. J. E. Whitfield.
Nos. 6, 10, 11, 18, 19	Dr. W. H. Melville.

TABLE X.

Analyses Nos. 10, 11, 12	Dr. W. H. Melville.

8.	19.	20.	21.	22.	23,	24.	25.	26.	27.
6.05	67.49	67.54	69.24	70.92	71.85	72.59	74.70	75.50	75.52
1.100	1.125	1.125	1.154	1.182	1.197	1.209	1.245	1.258	1.259
.34	.13	.80	.65	....	.43	.52			
6.96	16.18	17.02	15.30	13.24	13.17	13.47	13.72	13.25	14.11
.164	.157	.165	.148	.129	.129	.132	.134	.129	.138
2.59	1.30	2.97	1.72	3.54	2.17	1.58	1.01	1.02	1.74
.016	.008	.018	.010	.022	.013	.009	.006	.006	.011
1.38	1.22	.34	.69	.66	1.34	1.32	.62	.91	.08
.019	.017	.004	.009	.009	.018	.018	.009	.012	.001
one	.08	tr.	tr.	.14	.12	tr.	tr.		
3.37	2.68	2.94	2.98	1.42	2.25	2.12	.78	.90	.78
.060	.048	.053	.053	.025	.040	.039	.014	.016	.014
2.08	1.34	1.51	.95	.23	.63	1.05	.14	.07	.10
.052	.033	.038	.024	.006	.016	.026	.003	.002	.002
one	....	.03	none	....	....	....	....	.06	
4.20	4.37	4.62	4.46	4.28	4.06	4.63	3.90	4.76	3.92
.067	.070	.074	.071	.069	.065	.074	.062	.077	.063
2.53	2.40	2.28	2.52	4.25	3.89	2.52	4.02	2.85	3.63
.026	.025	.024	.026	.045	.041	.027	.042	.030	.038
r.	.13	tr.	tr.	.18	.14				
.03	....	.26	.27						
....	....	....	....	....	....	.26	.40	.32	.11
r.	....	.15	tr.						
.69	2.69	.55	1.30	.57	.43	.18	.62	.41	.39
0.22	100.01	101.01	100.08	99.59	100.48	100.24	99.91	100.05	100.38
....	....	.03							
		100.98							

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.
SiO_2	55.83	55.92	56.28	56.61	57.17	57.38	58.05	60.30	61.22	64.07	64.27	64.85	65.11	65.50
	.930	.932	.938	.943	.953	.956	.967	1.005	1.020	1.068	1.071	1.080	1.085	1.091
TiO_2	1.05	.94	.84	.79	1.03	tr.	1.05	.76	.61	.45	.32	.91	.71	.45
Al_2O_3	17.11	17.70	14.23	13.62	17.25	16.86	18.00	16.31	16.14	15.82	17.84	16.57	16.21	14.94
	.166	.171	.136	.132	.167	.163	.174	.158	.156	.153	.173	.160	.157	.145
Cr_2O_3	....	....	....	.05										
Fe_2O_3	4.07	3.16	4.69	5.89	2.48	2.49	2.49	4.35	3.01	3.40	3.36	2.10	1.06	1.72
	.025	.019	.029	.036	.015	.015	.015	.027	.018	.021	.021	.013	.006	.010
FeO	3.75	4.48	4.05	2.60	4.31	5.17	4.56	1.41	2.58	1.44	1.29	2.15	3.19	2.27
	.052	.062	.056	.036	.059	.072	.063	.019	.035	.020	.017	.029	.044	.031
MnO	none	tr.	.16	.35	none	tr.	none	.13	tr.	tr.	none	none	none	.20
NiO	....	....	....	....	....	....	....	....	.09	.05				
CaO	7.40	5.90	7.94	6.61	6.61	7.32	6.17	5.62	5.46	4.43	3.42	4.01	3.97	2.33
	.132	.105	.141	.117	.117	.130	.110	.100	.097	.079	.061	.071	.070	.041
BaO	....	....	....	....	....	....	....	.15	....	....	....	....	....	.13
MgO	5.05	4.34	6.37	5.48	4.83	5.51	3.55	2.39	4.21	3.39	2.00	2.14	2.57	2.97
	.126	.108	.159	.137	.120	.138	.089	.059	.105	.084	.050	.053	.064	.074
Li_2O	none	.09	.01	....	tr.	.39	none	....	....	....	.03	none	.04	....
Na_2O	2.95	4.08	2.98	3.13	3.44	3.33	3.64	3.99	4.48	4.06	3.84	3.71	4.00	5.46
	.047	.065	.048	.050	.055	.053	.058	.064	.072	.065	.061	.059	.064	.088
K_2O	1.71	2.28	1.23	2.71	2.03	1.45	2.18	2.36	1.87	2.27	2.48	3.10	2.51	2.76
	.018	.024	.013	.028	.021	.015	.023	.025	.019	.024	.026	.032	.026	.029
P_2O_5	.21	.18	.40	.06	.05	tr.	.17	.20	.25	.18	.16	.14	.02	.09
SO_3	tr.	tr.	tr.	(?)	tr.	.21	.07	.10	....	....	tr.	tr.	tr.	.06
FeS_2	....	....	....	....	....	....	....	....	....	....	....	....	....	....
Cl	none	none	.17	....	....	.17	tr.	....	....	....	none	none	none	....
H_2O	1.28	1.42	.93	2.27	1.20	.42	.86	2.50	.44	.52	1.32	.35	.94	1.37
	100.40	100.45	100.28	100.31	100.40	100.70	100.79	100.57	100.36	100.08	100.33	100.03	100.33	100.25
Less O for Cl	....	....	.04	....	....	.04	....	....	....	....	....	....	....	....
			100.24			100.66								

	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.	25.	26.	27.
SiO_2	65.60	65.66	65.97	66.05	67.49	67.54	69.24	70.92	71.85	72.59	74.70	75.50	75.52
	1.093	1.094	1.099	1.100	1.125	1.125	1.154	1.182	1.197	1.209	1.245	1.258	1.259
TiO_2	.75	1.37	.42	.34	.13	.80	.65	....	.43	.52			
Al_2O_3	17.61	15.61	16.53	16.96	16.18	17.02	15.30	13.24	13.17	13.47	13.72	13.25	14.11
	.170	.151	.160	.164	.157	.165	.148	.129	.129	.132	.134	.129	.138
Cr_2O_3													
Fe_2O_3	.95	2.10	2.59	2.59	1.30	2.97	1.72	3.54	2.17	1.58	1.01	1.02	1.74
	.006	.013	.016	.016	.008	.018	.010	.022	.013	.009	.006	.006	.011
FeO	2.76	2.07	1.72	1.38	1.22	.34	.69	.66	1.34	1.32	.62	.91	.08
	.038	.028	.024	.019	.017	.004	.009	.009	.018	.018	.009	.012	.001
MnO	none	....	none	none	.08	tr.	tr.	.14	.12	tr.	tr.		
NiO													
CaO	3.72	3.64	3.37	3.37	2.68	2.94	2.98	1.42	2.25	2.12	.78	.90	.78
	.066	.065	.060	.060	.048	.053	.053	.025	.040	.039	.014	.016	.014
BaO													
MgO	1.49	2.46	2.11	2.08	1.34	1.51	.95	.23	.63	1.05	.14	.07	.10
	.037	.061	.053	.052	.033	.038	.024	.006	.016	.026	.003	.002	.002
Li_2O	.03	.36	.09	none	....	.03	none	....	....	....	....	.06	
Na_2O	4.36	3.65	3.41	4.20	4.37	4.62	4.46	4.28	4.06	4.63	3.90	4.76	3.92
	.070	.058	.055	.067	.070	.074	.071	.069	.065	.074	.062	.077	.063
K_2O	.236	2.03	2.67	2.53	2.40	2.28	2.52	4.25	3.89	2.52	4.02	2.85	3.63
	.025	.021	.028	.026	.025	.024	.026	.045	.041	.027	.042	.030	.038
P_2O_5	.16	tr.	tr.	tr.	.13	tr.	tr.	.18	.14				
SO_3	tr.	.13	.13	.03	....	.26	.27						
FeS_2	....	....	....	....	....	....	....	....	....	.26	.40	.32	.11
Cl	....	.12	.09	tr.	....	.15	tr.						
H_2O	.59	1.07	1.23	.69	2.69	.55	1.30	.57	.43	.18	.62	.41	.39
	100.38	100.27	100.33	100.22	100.01	101.01	100.08	99.59	100.48	100.24	99.91	100.05	100.38
Less O for Cl	....	.03	.02	....	....	.03							
		100.24	100.31			100.98							

SiO_2
TiO_2
Al_2O_3
Fe_2O_3
FeO
MnO
NiO
CaO
MgO
Na_2O
K_2O
P_2O_5
Cl
H_2O

TABLE II.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.
SiO_2	51.81	52.09	52.11	53.56	53.71	53.89	55.93	56.21	57.26	57.32	57.64	61.16	63.42	63.97	64.40	71.62
	.863	.868	.868	.892	.895	.898	.932	.937	.954	.955	.960	1.019	1.057	1.066	1.073	1.193
TiO_2	.77	.39	.53	.68	.74	.49	.81	.88	.76	.62	.77	.23	.35	.48	.40	.08
Al_2O_3	15.24	17.84	16.58	16.07	18.00	18.81	18.32	18.24	19.40	17.29	18.43	16.17	17.16	15.78	15.77	14.99
	.147	.173	.160	.156	.174	.182	.177	.177	.188	.167	.178	.157	.166	.153	.153	.145
Fe_2O_3	3.66	4.27	3.66	3.21	3.99	4.92	2.39	3.26	2.49	3.89	3.63	2.89	3.09	2.35	2.47	1.27
	.023	.026	.023	.020	.024	.030	.014	.020	.015	.024	.023	.018	.019	.014	.015	.008
FeO	4.86	4.56	4.99	5.29	4.05	2.81	4.91	3.69	3.29	3.03	2.84	2.18	1.50	1.87	1.15	1.01
	.067	.063	.069	.073	.056	.039	.068	.051	.045	.042	.039	.030	.020	.026	.016	.014
MnO	.08	.14	.23	.11	.24	.17	.14	.17	.16	.06	.10	tr.	.04	.05	.04	.17
NiO	....	....	....	....	....	....	....	....	....	.10	....	....	.19	tr.	.17	
CaO	9.06	8.03	6.43	8.77	6.88	5.42	6.17	5.91	5.68	5.81	5.49	4.26	4.65	3.71	3.54	1.33
	.161	.143	.114	.156	.123	.096	.110	.105	.101	.103	.098	.076	.083	.066	.063	.023
MgO	8.89	5.33	6.87	7.23	5.19	3.29	3.97	3.38	2.57	3.50	3.32	3.89	1.64	2.84	2.12	.74
	.222	.133	.171	.180	.129	.082	.099	.084	.064	.087	.083	.097	.041	.071	.053	.018
Na_2O	2.83	3.39	3.25	3.06	3.50	3.65	4.29	4.15	4.21	3.89	4.03	3.87	4.51	4.36	4.10	3.62
	.045	.054	.052	.049	.056	.058	.069	.066	.067	.062	.065	.062	.072	.070	.066	.058
K_2O	2.08	1.98	3.20	1.94	3.10	2.98	2.62	3.02	2.95	3.04	3.33	3.20	3.04	4.01	3.81	4.81
	.022	.021	.033	.020	.032	.031	.027	.032	.031	.032	.035	.033	.032	.042	.040	.051
P_2O_5	.18	.27	.63	.18	.38	.52	.56	.64	.51	.50	.34	.13	.26	.40	.16	tr.
Cl	....	....	....	....	....	....	....	....	....	....	tr.					
H_2O	.67	1.77	1.99	.19	.55	2.99	.22	.78	.86	.63	.51	2.09	.44	.58	2.24	.41
	100.13	100.06	100.47	100.29	100.33	99.94	100.33	100.33	100.14	99.74	100.43	100.07	100.29	100.40	100.37	100.05

		16.	17.	18.	19.	20.
SiO_2 . .	9	54.86	56.05	57.29	60.89	69.45
	91	.914	.934	.954	1.014	1.157
TiO_2 . .	1	.69	.98	.72	.49	.19
Al_2O_3 .	9	17.28	19.70	18.45	17.14	14.92
	68	.168	.193	.179	.166	.146
Fe_2O_3 .	3	4.08	3.74	4.38	3.32	3.16
	29	.025	.023	.027	.020	.019
FeO . .	5	2.28	2.32	1.20	.95	.23
	45	.031	.032	.016	.013	.003
MnO .	4	.19	tr.	tr.	.09	.07
NiO . .	. .	. . . .	. . . .	.12	.19	
BaO . .	6	.37	. . . .	. . . .	. . . .	.03
CaO . .	4	5.42	4.34	3.57	3.58	1.19
	13	.096	.077	.063	.063	.021
MgO .	2	4.19	2.51	2.08	1.16	.05
	10	.105	.063	.052	.029	.001
Li_2O .	. .	. . . .	.06			
Na_2O .	3	3.94	3.29	4.43	4.54	3.19
	52	.063	.053	.071	.073	.051
K_2O . .	6	3.96	4.44	5.43	5.71	5.95
	41	.042	.047	.057	.060	.063
P_2O_5 . .	3	.48	.66	.46	.27	.06
SO_3 . . .	. .	. . . .	.19			
Cr_2O_3 .						
Cl						
H_2O . .	17	2.16	1.86	2.18	1.61	1.69
Le	02	99.90	100.14	100.31	99.94	100.18

TABLE III.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
SiO_2	47.28	48.36	48.95	49.71	50.06	50.99	51.46	51.76	51.82	52.33	52.37	52.49	52.63	52.93	53.49	54.86	56.05	57.29	60.89	69.45
	.788	.806	.812	.829	.834	.849	.857	.862	.863	.872	.873	.875	.877	.882	.891	.914	.934	.954	1.014	1.157
TiO_2	.88	1.18	.49	1.57	.51	.67	.83	.47	.71	.71	.73	.81	.81	.72	.71	.69	.98	.72	.49	.19
Al_2O_3	11.56	12.42	12.98	13.30	17.00	15.62	18.32	12.36	16.75	18.70	16.57	17.89	16.87	19.67	17.19	17.28	19.70	18.45	17.14	14.92
	.112	.121	.126	.130	.165	.151	.177	.120	.164	.181	.160	.175	.165	.192	.168	.168	.193	.179	.166	.146
Fe_2O_3	3.52	5.25	3.63	4.41	2.96	8.47	4.61	4.88	4.56	4.95	6.34	5.76	4.52	3.07	4.73	4.08	3.74	4.38	3.32	3.16
	.022	.032	.022	.027	.018	.053	.024	.030	.028	.031	.039	.036	.028	.019	.029	.025	.023	.027	.020	.019
FeO	5.71	2.48	4.68	3.37	5.42	1.43	2.71	4.60	3.36	1.83	2.35	2.08	3.11	3.50	3.25	2.28	2.32	1.20	.95	.23
	.079	.034	.065	.046	.075	.019	.037	.063	.046	.025	.032	.029	.043	.048	.045	.031	.032	.016	.013	.003
MnO	.13	.13	.13	.17	.14	tr.	.17	.11	.23	.03	.07	.09	.10	.15	.14	.19	tr.	tr.	.09	.07
NiO	....	....	....	....	....	.07	....	....	....	.14	.12	....	....	....	....	....	....	.12	.19	
BaO	....	.29	....	.46	....	....	....	....	.26	....	....	.30	.29	.21	.06	.37	....	....	....	.03
CaO	9.20	8.65	7.66	8.03	8.14	6.53	6.03	7.14	4.94	4.71	8.54	7.01	4.77	4.69	6.34	5.42	4.34	3.57	3.58	1.19
	.164	.154	.136	.143	.145	.116	.107	.127	.089	.084	.152	.125	.085	.083	.113	.096	.077	.063	.063	.021
MgO	13.17	9.36	11.73	7.96	3.61	5.23	2.91	9.57	4.03	2.69	5.27	3.49	3.69	2.88	4.42	4.19	2.51	2.08	1.16	.05
	.329	.234	.293	.199	.090	.130	.072	.239	.100	.067	.131	.087	.092	.072	.110	.105	.063	.052	.029	.001
Li_2O	none	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	.06			
Na_2O	2.73	1.46	2.31	1.49	3.53	3.39	4.11	1.99	3.91	4.51	2.99	3.18	3.86	4.20	3.23	3.94	3.29	4.43	4.54	3.19
	.044	.023	.037	.024	.056	.054	.066	.032	.063	.072	.048	.051	.062	.067	.052	.063	.053	.071	.073	.051
K_2O	2.17	3.97	3.96	4.81	3.40	3.05	4.48	3.83	5.02	5.45	2.45	3.73	5.17	4.75	3.86	3.96	4.44	5.43	5.71	5.95
	.022	.042	.042	.051	.036	.032	.047	.040	.053	.057	.026	.039	.055	.050	.041	.042	.047	.057	.060	.063
P_2O_5	.59	.84	.67	.66	.66	.53	.86	.56	.52	.81	.31	.55	.63	.59	.43	.48	.66	.46	.27	.06
SO_3	none	....	....	....	....	....	....	....	....	....	....	....	....	....	....	....	.19			
Cr_2O_3	....	tr.	....	tr.																
Cl	.18																			
H_2O	2.96	5.54	3.16	4.07	4.85	3.87	3.89	3.05	3.97	3.45	2.22	2.63	3.65	2.73	2.17	2.16	1.86	2.18	1.61	1.69
	100.08	99.93	100.35	100.01	100.28	99.85	100.38	100.32	100.08	100.31	100.33	100.01	100.10	100.09	100.02	99.90	100.14	100.31	99.94	100.18
Less O for Cl....	.04																			
	100.04																			

	18.	19.	20.	21.	22.	23.	24.
Si	59.12	60.77	61.05	61.49	61.55	61.87	62.17
	.985	1.013	1.018	1.025	1.026	1.031	1.036
Ti							
A	21.46	19.83	18.35	20.02	17.81	18.33	20.83
	.210	.194	.179	.196	.174	.179	.204
Fe	2.68	4.14	4.21	3.11	3.01	3.23	2.26
	.016	.026	.026	.019	.018	.020	.014
F	2.72	2.43	2.12	2.72	2.60	2.51	2.12
	.037	.033	.029	.037	.036	.034	.029
M	....	tr.	.04	.01	tr.	.01	tr.
Ca	2.16	1.63	2.05	1.88	1.69	2.11	1.68
	.038	.029	.036	.033	.030	.037	.030
M	.84	.34	.90	.52	.47	.65	.45
	.021	.008	.022	.013	.012	.016	.011
N	3.78	4.90	5.94	3.39	4.08	5.07	4.40
	.061	.079	.096	.054	.066	.081	.071
K	7.66	6.27	5.28	7.13	7.51	6.51	6.76
	.081	.066	.054	.075	.079	.069	.072
P_2	tr.	tr.	tr.	.02	.01	tr.	.03
S							
Fl							
Cl	....	....	....	....	....	.32	tr.
H	.25	.24	.32	.46	.86	.46	.25
	100.67	100.55	100.26	100.75	99.59	101.07	100.95

TABLE IV.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.	24.
SiO_2	46.36	46.76	47.20	47.53	47.96	48.30	48.54	48.83	49.15	50.04	52.26	54.83	55.08	55.17	55.48	55.51	57.73	59.12	60.77	61.05	61.49	61.55	61.87	62.17
	.774	.779	.786	.792	.799	.805	.809	.814	.819	.834	.871	.914	.918	.919	.924	.925	.962	.985	1.013	1.018	1.025	1.026	1.031	1.036
TiO_2	.29	.26	.34	.25	.26	.27	.21																	
Al_2O_3	18.60	18.24	18.80	19.49	20.00	17.92	14.86	15.34	13.37	13.67	16.99	20.17	17.25	17.09	19.56	20.05	17.85	21.46	19.83	18.35	20.02	17.81	18.33	20.83
	.182	.178	.184	.191	.196	.175	.145	.150	.131	.134	.166	.197	.169	.167	.191	.196	.175	.210	.194	.179	.196	.174	.179	.204
Fe_2O_3	4.12	3.40	4.76	2.04	5.04	5.48	4.17	7.39	6.65	4.55	2.13	4.77	....	....	....	....	4.44	2.68	4.14	4.21	3.11	3.01	3.23	2.26
	.025	.021	.029	.013	.031	.034	.026	.046	.041	.028	.013	.029					.028	.016	.026	.026	.019	.018	.020	.014
FeO	4.94	5.47	3.80	5.24	3.54	4.75	4.82	3.34	5.88	4.96	5.22	3.86	9.33	8.54	4.99	5.32	3.90	2.72	2.43	2.12	2.72	2.60	2.51	2.12
	.068	.076	.052	.072	.049	.066	.067	.046	.084	.069	.072	.053	.129	.118	.069	.071	.054	.037	.033	.029	.037	.036	.034	.029
MnO	1.00	1.04	1.07	1.18	1.11	1.33	1.18	tr.	....	....	....	....	....	....	....	....	....	....	tr.	.04	.01	tr.	.01	tr.
CaO	9.09	10.32	8.95	10.09	7.47	8.97	11.89	13.63	10.73	10.86	6.64	4.12	7.34	5.42	2.60	3.78	3.65	2.16	1.63	2.05	1.88	1.69	2.11	1.68
	.162	.184	.159	.180	.133	.160	.212	.243	.191	.194	.118	.073	.131	.096	.046	.067	.065	.038	.029	.036	.033	.030	.037	.030
MgO	4.00	4.26	3.97	4.10	3.16	3.51	5.75	4.65	5.30	5.37	1.86	1.93	2.77	1.95	.53	1.22	1.77	.84	.34	.90	.52	.47	.65	.45
	.100	.106	.099	.101	.079	.088	.144	.116	.132	.134	.046	.048	.069	.049	.013	.030	.044	.021	.008	.022	.013	.012	.016	.011
Na_2O	2.96	2.69	2.46	2.67	2.85	2.61	2.71	1.41	3.08	3.17	1.63	3.04	1.86	3.94	3.14	4.03	3.77	3.78	4.90	5.94	3.39	4.08	5.07	4.40
	.048	.043	.039	.043	.046	.042	.043	.022	.049	.051	.026	.049	.030	.063	.050	.065	.061	.061	.079	.096	.054	.066	.081	.071
K_2O	7.18	6.54	7.59	7.12	8.37	7.70	6.45	3.68	6.55	6.73	8.83	7.38	5.32	8.48	10.47	10.18	7.65	7.66	6.27	5.28	7.13	7.51	6.51	6.76
	.076	.069	.081	.075	.089	.082	.068	.039	.069	.071	.096	.078	.056	.090	.111	.108	.081	.081	.066	.054	.075	.079	.069	.072
P_2O_5	tr.	tr.	tr.	.19	tr.	tr.	.18	1.83	....	....	.94	....	....	....	....	....	tr.	tr.	tr.	tr.	.02	.01	tr.	.03
SO_3	....	....	....	....	....	....	....	.02	....	....	.22													
Fl	.06	tr.	tr.	....	tr.	tr.																		
Cl	tr.	tr.	tr.	....	tr.	tr.	....	tr.	....	....	.42	....	....	....	....	....	....	....	....	....	....	....	.32	tr.
H_2O	.40	.20	.16	.48	.16	.16	.16	....	....	....	3.33	.46	.17	....	.24	....	.09	.25	.24	.32	.46	.86	.46	.25
	99.00	99.18	99.10	100.38	99.92	101.00	100.92	100.12	100.71	99.35	100.47	100.56	99.12	100.59	100.01	100.09	100.85	100.67	100.55	100.26	100.75	99.59	101.07	100.95

		16.	17.	18.	19.	20.
SiO_2	30	50.61	51.73	52.09	53.36	55.66
	338	.843	.862	.868	.889	.928
TiO_2	38	.51	.54	.37	....	.33
Al_2O_3	67	15.67	14.68	16.58	11.47	11.03
	143	.153	.144	.162	.112	.108
Fe_2O_3	...	....	....	....	9.99	
					.062	
FeO	05	10.67	11.23	11.61	3.18	10.83
	167	.148	.156	.161	.044	.150
MnO	49	.52	.71	.32	....	.43
CaO	72	11.35	9.78	9.13	10.01	11.13
	191	.202	.174	.163	.178	.198
MgO	35	5.10	4.11	3.13	6.89	4.76
	134	.127	.103	.078	.172	.119
Na_2O	63	3.42	2.98	2.47	1.40	2.37
	043	.055	.048	.039	.022	.038
K_2O	56	.67	1.24	.89	2.81	.48
	006	.007	.013	.009	.029	.005
P_2O_5	21	1.80	3.65	3.22	.58	3.67
SO_3	11	.15	.21	.16	....	.06
Cl		....	....	....	....	tr.
H_2O	21	.10	.48	.64	.72	.26
	68	100.57	101.34	100.61	100.41	101.01

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
SiO_2	46.05	46.25	47.91	48.10	48.47	49.17	49.24	49.35	49.51	49.66
	.767	.771	.798	.801	.808	.819	.820	.823	.825	.828
TiO_2	.47	1.16	1.80	tr.	....	.49	....	....	.71	.67
Al_2O_3	19.29	21.54	19.44	21.61	22.67	20.53	19.06	15.71	16.32	18.08
	.188	.211	.190	.211	.222	.201	.187	.154	.160	.177
Fe_2O_3	....	5.00	5.33	3.13	2.76	....	1.77	7.44	8.38	....
		.031	.033	.019	.017		.011	.046	.052	
FeO	11.18	7.29	6.17	7.72	7.33	10.64	10.33	6.96	4.46	12.07
	.155	.101	.086	.107	.102	.148	.143	.097	.062	.167
MnO	.59	....	....	....	....	.58	....	....	....	.63
CaO	10.81	10.20	9.94	8.82	10.72	9.81	8.75	9.80	12.44	9.60
	.193	.182	.177	.157	.191	.175	.156	.175	.222	.171
MgO	3.69	2.03	2.15	2.78	2.18	2.21	5.00	5.71	4.56	4.21
	.092	.051	.054	.069	.054	.055	.125	.142	.114	.105
Na_2O	3.55	3.97	5.57	4.19	4.50	2.89	3.89	2.96	1.64	2.49
	.057	.064	.089	.067	.071	.046	.064	.047	.026	.040
K_2O	.73	2.62	1.72	4.05	1.59	.88	1.19	1.31	.63	.84
	.007	.028	.018	.043	.017	.009	.012	.014	.006	.009
P_2O_5	2.65	....	....	....	....	3.21	....	....	1.23	1.71
SO_3	.23	....	....	....	....	.09	....	....	.07	.11
Cl	tr.	....	....	....	....	tr.	....	....	....	....
H_2O	1.03	....	....	....	....	.22	.63	.49	.12	.37
	100.27	100.06	100.03	100.40	100.22	100.72	99.89	99.73	100.07	100.44

	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
SiO_2	49.87	49.93	50.03	50.23	50.30	50.61	51.73	52.09	53.36	55.66
	.831	.832	.834	.837	.838	.843	.862	.868	.889	.928
TiO_2	....	....	.70	.31	.38	.51	.54	.37	....	.33
Al_2O_3	14.80	17.72	17.71	15.55	14.67	15.67	14.68	16.58	11.47	11.03
	.145	.173	.173	.152	.143	.153	.144	.162	.112	.108
Fe_2O_3	8.25	7.44	6.30	....	....	....	....	....	9.99	
	.051	.046	.039						.062	
FeO	6.88	3.61	6.59	10.48	12.05	10.67	11.23	11.61	3.18	10.83
	.094	.050	.091	.145	.167	.148	.156	.161	.044	.150
MnO	....	....	....	.52	.49	.52	.71	.32	....	.43
CaO	9.36	10.64	9.76	11.73	10.72	11.35	9.78	9.13	10.01	11.13
	.167	.190	.174	.209	.191	.202	.174	.163	.178	.198
MgO	6.77	3.49	2.65	5.09	5.35	5.10	4.11	3.13	6.89	4.76
	.169	.087	.066	.127	.134	.127	.103	.078	.172	.119
Na_2O	2.81	2.66	3.10	2.78	2.63	3.42	2.98	2.47	1.40	2.37
	.045	.043	.050	.044	.043	.055	.048	.039	.022	.038
K_2O	.68	1.87	3.30	.51	.56	.67	1.24	.89	2.81	.48
	.007	.019	.035	.005	.006	.007	.013	.009	.029	.005
P_2O_5	....	1.66	....	3.16	3.21	1.80	3.65	3.22	.58	3.67
SO_3	....	....	....	.08	.11	.15	.21	.16	....	.06
Cl	....	....	....	tr.	tr.	....	....	....	....	tr.
H_2O	.45	1.72	....	.45	.21	.10	.48	.64	.72	.26
	99.87	100.74	100.14	100.89	100.68	109.57	101.34	100.61	100.41	101.01

TABLE VI.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
SiO_2	60.24	61.43	61.47	67.18	67.48	67.89	68.33	68.75	69.02	69.61	70.30
	1.004	1.024	1.024	1.119	1.124	1.131	1.139	1.146	1.150	1.160	1.171
Al_2O_3	20.28	17.51	18.09	14.18	6.70	11.53	10.94	5.91	10.09	8.02	6.32
	.198	.171	.177	.139	.065	.113	.107	.058	.099	.078	.062
Fe_2O_3	2.32	5.11	5.14	4.00	7.42	4.51	3.74	5.81	4.42	7.17	9.23
	.014	.032	.032	.025	.046	.028	.023	.036	.028	.045	.058
FeO	3.88	2.30	3.06	2.48	2.21	4.52	5.41	5.33	4.56	2.83	1.40
	.054	.032	.042	.034	.030	.062	.075	.074	.063	.039	.019
CuO	....	....	....	....	....	....	.25	.23	.29	....	....
CaO	1.96	2.45	3.00	2.78	1.45	1.51	1.36	2.11	1.45	.88	.84
	.035	.043	.053	.049	.026	.027	.024	.038	.026	.016	.015
MgO	.50	.54	1.32	.34	.77	.62	.16	.08	.76	.65	.89
	.012	.013	.033	.008	.019	.015	.004	.002	.019	.016	.022
Na_2O	7.80	6.22	5.85	5.89	7.21	5.79	7.09	7.52	6.29	7.47	7.70
	.126	.100	.094	.095	.116	.093	.114	.121	.101	.120	.124
K_2O	4.28	3.95	2.83	4.01	2.94	3.71	4.08	4.28	3.70	2.88	2.50
	.045	.042	.029	.042	.031	.039	.043	.045	.039	.030	.026
H_2O	....	....	....	....	.96	.33	....	....	....	.74	.82
	101.26	99.51	100.76	100.86	100.14	100.41	101.36	100.02	100.58	100.25	100.00

Showing Range in Silica of Lamprophyres and their Equivalent Surface Rocks.

Lamprophyres.		Volcanic Rocks.	
SiO_2	Rock.	SiO_2	Rock.
40.95	Camptonite	39.29	Nephelinite
41.43	Minette	40.26	Leucite-basalt
41.63	Camptonite	41.01	Hornblende-basalt
41.94	Camptonite	42.21	Nephelinite
42.07	Minette	42.30	Nephelinite
42.77	Camptonite	42.68	Hornblende-basalt
44.94	Minette	42 69	Nephelinite
45.14	Kersantite	42.92	Leucite-basalt
45 93	Minette	42.92	Hornblende-basalt
46.00	Kersantite	43.90	Nephelinite
46.17	Kersantite	44.04	Basalt
47.56	Minette	44.14	Hornblende-basalt
47.64	Kersantite	44 35	Leucite-basalt
47.73	Minette	44.62	Leucite-basalt
47.88	Minette	44.80	Nephelinite
47.99	Minette	45.04	Nephelinite
48.43	Syenite	46.41	Leucitophyre
48.57	Minette	46.97	Nephelinite
49.01	Minette	47.16	Leucitophyre
49.16	Kersantite	47.17	Leucitophyre
49 50	Kersantite	47.44	Basalt
49.52	Kersantite	47.64	Basalt
49.82	Kersantite	47.78	Leucitophyre
50.80	Kersantite	48.17	Leucitophyre
51.15	Minette	48.21	Nephelinite
51.64	Minette	48.27	Nephelinite
51.94	Kersantite	48.28	Nephelinite
52.32	Minette	48.54	Leucitophyre
52.61	Kersantite	48.68	Leucitophyre
52.68	Kersantite	49.15	Leucitophyre
53.16	Kersantite	50 75	Basalt
53.26	Kersantite	50.99	Hornblende-andesite
53.40	Kersantite	51.12	Basalt
53.42	Minette	51.41	Trachyte
54.25	Kersantite	51.51	Mica-melaphyre
54.80	Kersantite	52.37	Basalt
54.94	Kersantite	52.42	Augite-andesite
55.06	Kersantite	52.58	Augite-andesite
55.76	Minette	52.69	Mica-melaphyre
56.37	Minette	53.17	Basalt
56.80	Minette	53.22	Mica-melaphyre
57.05	Minette	53.33	Trachyte
57.30	Minette	53.50	Basalt
57.37	Minette	53.75	Basaltic andesite
58.25	Syenite	53.81	Basalt
58.34	Minette	54.10	Basalt
60 02	Minette	54.39	Basalt
61.12	Minette	54.80	Basalt
62.00	Kersantite	54 82	Melaphyre
63.61	Kersantite	55.00	Basalt
67.28	Minette	55 08	Trachyte
		55.35	Mica-basalt
		57.04	Mica-basalt
		57.48	Mica-basalt
		58.30	Adesite
		59 21	Porphyrite

TABLE VIII.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
SiO_2	41.43	42.07	44.94	45.93	47.56	47.73	48.43	51.15	51.64	55.76	57.37	58.34
Al_2O_3	16.46	14.78	10.77	11.88	12.69	10.07	11.41	15.91	14.12	15.87	13.84	16.33
Fe_2O_3	8.78	8.64	6.95	1.17	5.26	7.39	12.32	4.63	....	....	2.44	2.28
FeO	3.16	3.00	6.61	8.72	3.35	4.29	.64	3.72	9.55	7.87	3.44	3.38
MnO	.19	tr.	tr.	.47	1.34	.23	.34	....	....	.19	tr.	.14
CaO	10.05	8.17	9.96	11.85	8.38	6.97	9.97	7.68	6.13	6.23	5.53	5.65
MgO	9.83	8.31	10.39	9.97	10.91	7.66	8.23	4.14	6.17	5.44	6.05	3.34
Na_2O	.63	1.90	.43	1.90	2.33	3.78	3.59	1.92	2.38	2.10	1.53	2.20
K_2O	1.24	1.69	5.17	3.28	3.98	1.22	3.21	5.97	3.47	4.01	4.47	5.55
P_2O_5	.65	.57	.93	1.66	.91							
Fl	....	....	....	.97	....	....	....	....	....	....	.37	
S	....	....	....	.48								
FeS_2	....	....	....	....	....	....	....	....	....	....	1.13	
CO_2	5.15	7.23	2.47	2.69	.88	5.88	tr.	2.12	3.09	2.03	.67	.66
H_2O	1.83	2.42	2.68	.67	2.16	4.46	1.33	2.75	4.33	1.68	3.17	2.35
	99.40	98.78	101.30	101.64	99.75	99.68	99.47	99.99	100.98	101.18	100.01	100.72

TABLE IX.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.
SiO_2....	40.95	41.63	41.94	42.77	42.03	43.50	43.74	45.14	46.48	49.50	49.82	52.61	54.80	54.94	62.00
TiO_2....	3.39	3.95	4.15	2.35	3.70	2.10	2.80	....	.99	....	....	.50	.30	....	.20
Al_2O_3...	16.45	13.26	15.36	14.06	13.60	18.06	14.82	9.68	16.16	13.09	14.50	14.64	11.20	7.69	10.96
Fe_2O_3...	13.47	3.19	3.27	2.72	17.55	7.52	2.40	13.55	6.17	2.63	8.06	1.36	3.42	9.58	5.54
FeO....	....	9.92	9.89	8.34	6.65	7.64	7.52	....	6.09	6.20	....	6.75	4.80	4.37	4.41
MnO...	.33	.27	.25	.15	tr.	....	....	tr.	....	.19	....	.30	.40	1.53	.41
CaO....	10.53	8.86	9.47	11.47	14.15	13.39	10.81	8.65	7.35	7.94	7.69	7.30	8.32	5.11	1.02
MgO...	6.10	7.31	5.01	9.72	6.41	3.47	6.98	10.78	4.02	8.97	5.81	7.20	6.00	3.03	6.52
Na_2O...	4.00	2.29	5.15	1.89	1.83	2.00	3.08	2.02	5.85	2.60	3.03	.82	1.15	2.47	1.78
K_2O....	1.28	3.32	.19	1.43	9.97	1.30	2.90	3.27	3.08	4.52	3.50	.95	1.25	4.03	2.46
P_2O_5....	.29	....	....	....	.67	....	.64	tr.	....	1.29	tr.	.30	.40	.91	.35
NaCl...	....	....	....	....	.05	....	tr.	....	....	....	....	....	....	.22	
Cr_2O_3...	....	....	....	....	....	....	....	....	....	....	....	.35	.05	....	.05
SO_3.....	....	....	....	....	.08										
FeS_2....	....	....	....	....	....	....	.10								
S.......	....	....	....	....	....	....	....	tr.	....	tr.	tr.	.10	....	.09	
Cu......	....	....	....	....	....	....	....	....	....	tr.	....	.24			
CO_2....	....	5.20	2.47	1.62	....	....	1.50	2.82	.45	1.20	4.42	3.40	4.63	4.32	.44
H_2O....	3.84	1.35	3.29	2.74	1.08	1.22	2.94	4.24	4.27	.55	2.54	2.70	2.82	1.49	2.99
	100.63	100.75	100.44	99.26	99.23	100.20	100.23	100.15	100.91	98.90	99.37	99.52	99.54	99.78	99.13

TABLE X.

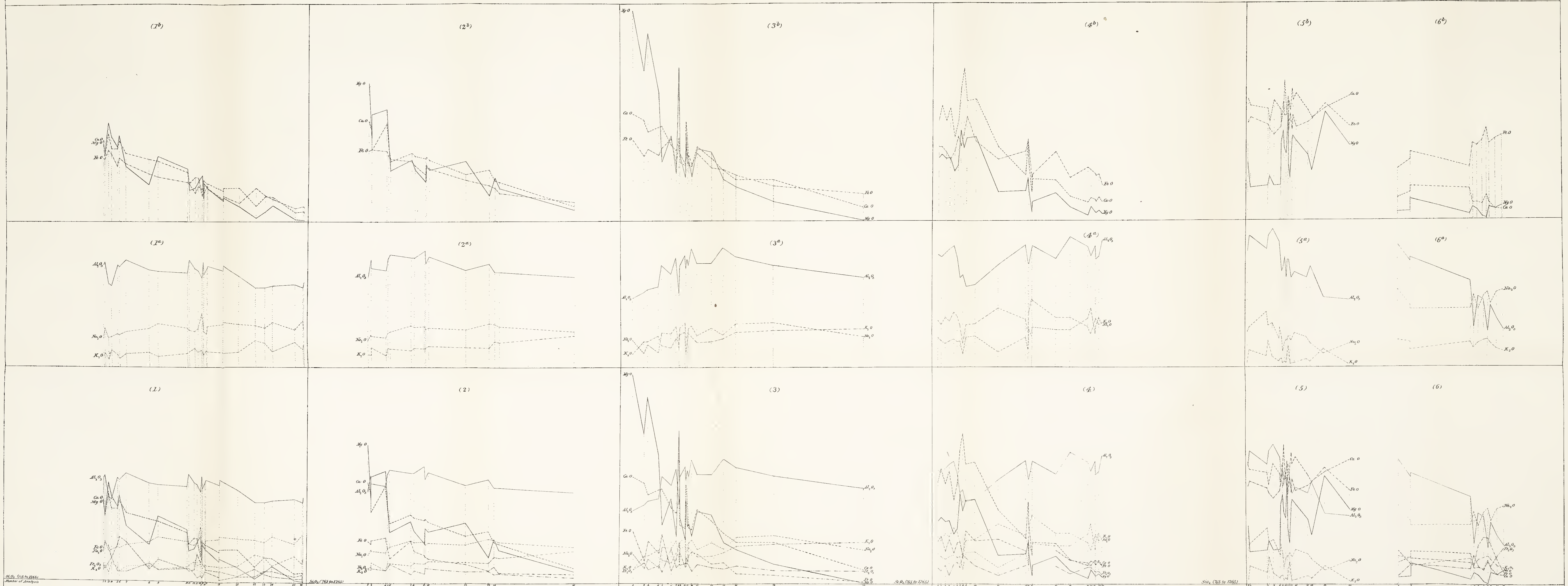
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.
SiO_2	42.92	44.35	47.64	51.51	52.64	52.69	53.22	54.82	55.00	55.35	57.04	57.48	58.30
TiO_2	....	....	.48	....	1.08	....	....	....	....	.87	.94	.94	
Al_2O_3	13.61	10.20	13.86	13.81	10.42	16.61	14.15	16.02	14.38	12.71	13.66	14.09	17.39
Fe_2O_3	16.28	13.50	6.72	7.00	7.36	7.60	8.34	2.02	....	4.67	4.96	5.21	5.47
FeO	....	....	7.94	....	5.74	....	....	6.99	9.29	2.06	1.77	1.35	.93
MnO	....	....	....	1.06	....	.98	.50	....	....	.08	.17	.09	tr.
$\left.\begin{matrix}Ni\\Co\end{matrix}\right\}O$	....	....	....	....	....	....	....	....	....	.05	.07	.08	
BaO	....	....	....	....	....	....	....	....	....	.19	.22	.23	
CaO	10.75	11.47	10.68	5.61	9.42	1.40	4.80	5.76	8.51	5.77	6.23	6.05	5.11
MgO	11.36	12.31	9.03	5.15	6.87	7.12	5.34	5.07	7.72	6.29	4.43	3.49	2.36
Na_2O	1.94	3.37	1.28	1.87	4.24	.39	2.23	2.26	2.25	2.65	3.08	3.00	4.35
K_2O	3.02	4.42	2.70	5.57	2.08	8.03	5.27	2.66	2.52	4.86	4.95	4.69	4.04
P_2O_5	....	....	tr.	....	1.24	....	1.00	.36	....	.58	.63	.65	
CO_2	....	....	....	5.14	....	....	2.30	1.21					
H_2O	....	....	1.42	2.00	.22	5.95	2.30	2.16	.48	3.85	2.21	2.57	2.45
	99.88	99.62	101.75	98.72	101.31	100.77	99.45	99.33	100.15	99.98	100.36	99.92	100.40

TABLE XI.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.
SiO_2	41.01	42.68	42.21	42.65	42.69	42.92	48.54	52.58	53.25	53.33	54.10	54.39
TiO_2	.48	.51	4.48	....	....	....	.21					
Al_2O_3	11.58	9.42	9.95	15.35	14.14	13.61	14.86	15.51	14.14	18.21	11.82	10.09
Fe_2O_3	12.54	11.55	14.11	6.46	15.86	16.28	4.17	7.28	....	9.87	....	7.07
FeO	7.60	7.23	....	8.19	....	....	4.82	3.28	11.63	....	13.92	5.79
MnO	....	....	....	....	....	....	1.18	.35				
CaO	12.20	13.15	13.91	11.96	11.59	10.75	11.89	7.83	9.17	7.56	8.79	8.89
MgO	8.67	10.09	9.11	7.14	9.06	11.36	5.75	7.77	7.10	3.50	5.56	6.49
Na_2O	2.57	2.71	2.53	5.02	3.12	1.94	2.71	3.69	3.30	5.24	5.01	4.16
K_2O	1.45	1.16	3.55	1.47	1.75	3.02	6.45	.49	.62	3.42	.47	2.17
P_2O_5	.75	1.29	....	....	....	....	.18					
H_2O	1.87	1.06	....	1.28	1.71	....	.16	.40	.77	.45	1.41	.57
	100.72	100.85	99.85	99.52	99.92	99.88	100.92	99.18	99.98	101.58	101.08	99.62

$^{b)}$

DIAGRAMS OF MOLECULAR VARIATIONS IN CERTAIN GROUPS OF IGNEOUS ROCKS.



Electric Peak and Sepulchre Mountain. | Crandall Basin. | Exceptional rocks of the Absaroka Range. | Vesuvius, Rocca Monfina and Ischia. | Etna. | Pantelleria.

DETERMINATION OF THE DATES OF PUBLICATION OF CONRAD'S "FOSSILS OF THE TERTIARY FORMATION" AND "MEDIAL TERTIARY."

BY

WILLIAM HEALEY DALL.

[Read before the Society, November 12, 1892.]

For many years the paleontologists of Europe and America who desired to work on the Tertiary fossils of this country have been hampered by uncertainties of nomenclature due to want of information in regard to the dates of publication of the works above alluded to, as well as the scarcity of the works themselves. Some time since the writer began to collect data for use in his own work, finding the references already printed by Lea and Conrad, and subsequently by O. Meyer, insufficiently full to serve the desired purpose. Having enjoyed the personal acquaintance of both Conrad and Lea, more especially the latter—and of Meek and others who were more or less conversant with the events connected with the claims of priority made by both Conrad and Lea—it has seemed well to put on record a statement of the circumstances as the writer has been able to learn them, especially since they tend to show that the controversies which arose were due to misunderstandings and the injudicious meddling of well-meaning friends, rather than any intentional unfairness on the part of either of the principals.

In getting at the dates of publication of the different parts of Conrad's papers the writer has examined thirteen copies or parts of copies of the "Medial Tertiary," and has found that three others certainly exist which have not been acces-

sible. Of the "Fossils of the Tertiary formation" he has seen eight copies and found track of five more. In this work he has been much indebted to the energetic labors of Mr. Gilbert D. Harris, of the United States Geological Survey. Mr. Harris and the writer have co-operated, with the same end in view, in the hope that when the material was all brought together some means might be found for reprinting these papers so important for all students of American Tertiary Geology. Pending such a reprint the writer has prepared the present summary of the facts toward which the work of Mr. Harris equally with his own has contributed, but for all expressions of opinion here, in cases of doubt, the writer is alone responsible.

In January, 1829, Judge Charles Tait, of Claiborne (then called Fort Claiborne), Alabama, called the attention of Mr. Isaac Lea to the rich fossil fauna of the sandy stratum in the bluff at that place, and during subsequent correspondence supplied specimens of these fossils and a descriptive section of the bluff. Subsequently specimens of these fossils were sent to other geologists and excited much interest.

Dr. Gates, of New York, at the instance of several gentlemen, visited Claiborne in the season of 1831–'2, and made a collection which was sent to Philadelphia, and, through the initiative of Dr. Morton, was placed in the hands of Mr. Conrad to be determined; the new species comprised in it were described by him in 1832 in Nos. 1 and 2 of the "Tertiary fossils," though he does not mention from whom they were received.

In 1833 Mr. Conrad, then the most prominent student of the Tertiary of the United States, decided to make a journey in the south which should take him over the major part of the Tertiary belt of the United States, including a visit to Claiborne. As Mr. Conrad was in very moderate circumstances he invited subscriptions to sets of the fossils to be collected in order to obtain means for the journey. Mr. Lea, Dr. Morton, and other patrons of science in Philadelphia were among the subscribers. It was understood that Mr.

Conrad reserved the right to describe the new species he might collect, in the publication he had begun the previous year and of which two numbers had appeared.

Mr. Lea, having completed some work on the Unios upon which he was engaged, began to prepare for publication a paper on the fossils which he had received from Judge Tait.

Mr. Conrad appears to have spent little time elsewhere before proceeding to the rich deposit at Claiborne, from whence he sent descriptions of a number of new forms together with specimens to Dr. Morton and Mr. Say at Philadelphia. It is probable that these specimens were made up into sets and partly distributed to the subscribers and partly offered for sale. The descriptions were printed under the direction of Messrs. Say and Morton as No. 3 of the "Fossils of the Tertiary formations," and duplicate sets of the fossils were offered for sale by an advertisement on the back of the cover of that number.

About this time it became known that Mr. Lea was working on the Claiborne fauna, and the friends of Conrad, Messrs. Say and Morton, jumped to the conclusion that Mr. Conrad's rights were being invaded, though as a matter of fact Mr. Lea was working on material received before Conrad left for the south. Very hurriedly, to preserve the rights of priority for Conrad, Messrs. Say and Morton prepared brief diagnoses of many of Conrad's species and published them as No. 4 of the "Fossils of the Tertiary formation" between the time Mr. Lea presented his paper to the Academy of Natural Sciences and the date of the actual publication of Lea's volume.* Their publication was made in Conrad's name, although I have been assured that Conrad never saw a line of it until after the paper was printed and distributed. But, of course, as it was made by his patrons and intimate friends, Messrs. Morton and Say, and for the purpose of preserving his rights as they supposed, he shouldered the responsibility on his return; the more so, as it doubtless appeared to him that his friends

* Contributions to Geology, by Isaac Lea [etc.] Philadelphia: Carey, Lea and Blanchard, 1833. viii. 227 pp. 8°, with six plates.

were right in thinking that Mr. Lea was endeavoring to anticipate him.

On the other hand, Mr. Lea considered that he had a perfect right to publish on Tait's fossils without reference to Conrad's subsequent collections, and regarded the publication of No. 4 as an attempt to deprive him of the fruit of his labor and expense. The copies of No. 4 were bought up and destroyed as far as possible, and Mr. Lea, in accordance with a very ancient usage, claimed priority for his descriptions from the date of the presentation of his paper at the Academy, a priority which the friends of Conrad, in accordance with more modern custom, refused to admit. The result was a serious breach between the naturalists at Philadelphia, who were divided into the partisans of one or the other geologist, and the differences thus begun continued to exist for many years, with unfortunate results for science.

COLLATION OF THE WORKS REFERRED TO.

Vol. 1. No. 1. Fossil Shells | of | the Tertiary Formations | of North America. | Illustrated by figures drawn on stone | from nature. | By T. A. Conrad, | Member of the Acad. Nat. Sc. of Philada. | Contents. | Arca limula [15], tab. 1, fig. 1. A. transversa [15], tab. 1, fig. 2. A. stillicidium [15], tab. 1, fig. 3. A. centenaria [16], tab. 1, fig. 4. A. idonea [16], tab. 1, fig. 5. A. incile [16], tab. 2, fig. 1. Pectunculus pulvinatus [17], tab. 2, fig. 2. P. subovatus [17], tab. 2, fig. 3. Murex umbrifer [17], tab. 3, fig. 1. Fusus exilis [17], tab. 3, fig. 2. F. sulcosus [18], tab. 3. fig. 3. F. strumosus [18], tab. 3, fig. 4. Fusus trossulus [18], tab. 3, fig. 5. F. tetricus [18], tab. 3, fig. 6. F. rusticus [18], tab. 4, fig. 1. F. parilis [18], tab. 4, fig. 2. F. cinereus [19], tab. 4, fig. 3. Buccinum porcinum [19], tab. 4, fig. 4. B. laqueatum [19], tab. 4, fig. 5. B. altile [19], tab. 4, fig. 6. Cypricardia arata [20], tab. 5, fig. 1. Cardita planicosta [20], tab. 5, fig. 2. Artemis acetabulum [20], tab. 6, fig. 1.

| Philadelphia: | Sold by Judah Dobson, No. 108 Chestnut St. | October 1, 1832. |

The above is printed on the yellow paper cover of the first edition. The page references to the species have been added in brackets. The other pages of cover are blank. Then follows title page, as follows:

Fossil shells | of the | Tertiary formations | of | North America, | Illustrated by figures drawn on stone, | from nature. | — | By T. A. Conrad, | Member of the Acad. Nat. Sc. of Philada. | — | Vol. 1. | — | Philadelphia. | 1832. |

Then follows blank page; page [iii] with dedication to Samuel George Morton, M. D., signed T. A. C.; page [iv] with preface, which ends on page viii and is dated October 1, 1832. Pages 9 to 14 contain the "Introduction. Brief review of the Tertiary formations of the United States," and pages 15–20 descriptions of species, headed, page 15, "Fossil Shells, &c." Lastly, there are plates 1–6. The whole in 8°.

Mr. Meek has noted on his copy that Part 1 was distributed about July or August, 1832 (see Am. Journ. Sci., 1st ser., vol. 23, p. 204, Jan., 1833). But this, in view of the date of the introduction, must be regarded as applying only to proofs more or less complete of the pages of descriptions and the plates. The notice referred to is said to have been written by Dr. Morton.

Vol. 1. No. 2. [On yellow cover has title same as in No. 1, then follow contents:] Lucina acclinis [21], t. 6, fig. 2. Crassatella alta [21], t. 7, figs. C. Marylandica [22], t. 8, fig. 1. C. protexta [22], t. 8, fig. 2. C. undulata [23], t. 9, fig. 1. C. melina [23], t. 9, fig. 2. Turbinella pyruloides [24], t. 10, fig. 1. Ancillaria altile [24], t. 10, fig. 2. A. subglobosa [25], t. 10, fig. 3. A. scamba [25], t. 10, fig. 4. A. staminea [25], t. 10, fig. 5. Mactra delumbis [26], t. 11. Pholadomya abrupta [26], t. 12. Ostrea radians [27], t. 13, fig. 1. O. sellæformis [27], t. 13, fig. 2. O. Carolinensis [27], t. 14, fig. 1. O. Virginiana [28], t. 14, fig. 2.

| Philadelphia: | Sold by Judah Dobson, No. 108 Chestnut St. | December, 1832. | W. P. Gibbons, Pr. |

There is no title page, pages 21–28 and plates 7–14 follow. On the fourth page of cover is an advertisement, announcing

the author's proposed journey to the Southern States to collect fossils; that the third number will appear when the sale of the first shall warrant its publication, and the terms, one dollar per number.

Vol. 1. No. 3. [Yellow cover same as preceding, except list of contents and last two lines.] Voluta Sayana [p. 29]. V. petrosa [29]. Fusus trabeatus [29]. F. papillatus [29]. F. inauratus [29]. F. thoracicus [30]. Melongena armigera [30]. Murex engonatus [30]. Scalaria nassula [31]. Solarium alveatum [31]. S. antrosum [31]. Rostellaria velata [31]. Oliva Alabamensis [32]. O. bombylis [32]. Infundibulum urticosum [32]. Pyrula penita [32]. Fissurella tenebrosa [33]. Conus sauridens [33]. Marginella larvata [33]. M. crassilabra [33]. Dentalium thalloides [34]. Volvaria galba [34]. Mitra bolaris [34]. M. doliata [34]. Buccinum sagenum [34]. Cancellaria gemmata [35]. Littorina antiquata [35]. Melania vetusta [35]. Siliquaria vitis [36]. Cytherea Poulsoni [36]. C. Hydana [36]. C. æquorea [36]. C. discoidalis [37]. C. perovata [37]. Nucula magnifica [37]. Arca cuculloides [37]. Corbula nasuta [38]. Astarte callosa [38]. A. proruta [38]. Plicatula filamentosa [38].

| August, 1833. | W. P. Gibbons, Pr., cor. Sixth and Cherry Sts. |

There is no title page to Part 3, but pages 29 to 38 follow, and on the fourth page of the cover is the following note: "Persons desirous of having a series of tertiary fossils from Alabama and the other Southern States, embracing about eighty species, may obtain them on application to Wm. P. Gibbons, at his printing office, corner of Sixth and Cherry streets. Price of the collection, $15.

"The plates illustrative of this number are in progress and will be published with No. 4, which is nearly ready for the press.

"The length of the bivalves in this paper is measured from the anterior to the posterior extremity. August 24."

There are no plates in this number, though the plate ref-

erences accompany the descriptions in the text from plate 15, fig. 1, to plate 20 [misprinted 29], fig. 5. In the subsequent reprint plates 15 to 18 were supplied, but those numbered 19 and 20 were never published. In one of the copies of Conrad in the library of the Museum of Comparative Zoology, at Cambridge, Mass., there are bound up two plates numbered 19 and 20, which were probably proofs of plates intended to complete the series, but which were never issued. They illustrate some of the species in Part 3, but also some not mentioned there and not all of those which are mentioned in the original.

Number 3 was printed under Morton's supervision from manuscript forwarded from Conrad during his absence in the South. The specimens sent by Conrad were partly distributed to his subscribers to the collecting fund and the remainder offered to the public, as above noted. September 3, 1833, "Mr. Conrad's work, 'Fossil shells of the Tertiary formation,'" is noted on the records of the Philadelphia Academy as presented to their library. It is presumed that the gift included Nos. 1 to 3.

Part 4 appeared about two months after Mr. Lea had presented (August 27, 1833) a notice of his manuscript work to the Academy of Natural Sciences. Mr. Lea's work appeared early in December, 1833, about five weeks after the publication of Say and Morton's issue, and, of course, is subsequent in point of time; though the descriptions in Conrad's Part 4 are so brief—each rarely more than three lines of type, often without measurements and with no figures, quite unidentifiable in many cases without the specimens—that it is doubtful whether they are really entitled to priority, as against the much more careful and, for the period, admirably illustrated work of Lea. A copy of Lea's work was presented to the Academy by the author December 10, 1833, according to the manuscript record of the Society made at the time.

In Part 4 the general title heading the cover is the same as in the previous numbers. The cover is of yellow paper.

Vol. 1. No. 4. [Title omitted.] Contents. | Stalagmium [n. g.] margarataceum [39]. Avicula limula [39]. Pectunculus idoneus [39]. P. aviculoides [39]. P. declivis [39]. P. decisus [39]. P. corbuloides [40]. Lucina symmetrica [40]. L. subvexa [40]. L. pandata [40]. L. dolabra [40]. L. alveata [40]. L. carinifera [40]. L. pomilia [40]. Tellina alta [41]. T. papyria [41]. Corbis lamellosa [41]. C. distans [41]. C. undata [41]. Lutraria papyria [41]. Rostellaria laqueata [41]. Erycina æquorea [42]. E. rectilinearis [42]. Mactra decisa [42]. M. prætenuis [42]. M. parilis [42]. Donax limatula [42]. Amphidesma linosa [42]. Psammobia filosa [42]. P. eborea [42]. Fusus protextus [43]. F. rannelloides [43]. F. thalloides [43]. F. altilis [43]. F. explicatus [43]. F. stamineus [43]. F. proscissus [43]. F. bellus [43]. F. limulus [43]. F. prorutus [43]. F. decisus [43]. Emarginula arata [44]. Monoceros vetusta [44]. M. armigera [44]. Ancillaria lymneoides [44]. Solarium scrobiculatum [44]. S. stalagmium [44.] S. funginum [44]. S. exacuum [44]. S. amœnum [44]. Cancellaria alveata [45]. Actæon pomilius [45]. A. idoneus [45]. A. costellatus [45]. Sigaretus arctatus [45]. S. declivis [45]. Scalaria sessilis [45]. Turritella obruta [45]. Buccinum perlatum [45]. B. prorsum [45]. B. amænum [45]. Turbinella prisca [45] T. prætenuis [45]. Cassis nuperus [46]. Pleurotoma elaborata [46]. P. tabulata [46]. P. alternata [46]. P. depygis [46]. P. nupera [46]. Natica ætites [46]. N. eminula [46]. N. limula [46]. N. eborea [46]. Pyramidella larvata [46]. Mitra pactilis [46]. M. perexilis [46]. Nucula equalis [46]. N. opulenta [46].

| Philadelphia. | Sold by Judah Dobson, No. 108 Chestnut street. | October, 1833. | W. P. Gibbons, printer. |

On the last page of the cover is the following note: "The last number of this work was exclusively devoted to the fossil shells of the *Calcaire Grossier* or middle tertiary deposits of Alabama, and I now continue this interesting part of my subject. I have to regret the necessity of publishing

at the distance of a thousand miles from my present domicile; an arrangement that may possibly involve a few inaccuracies in my papers. But, as I hope to return to Philadelphia in the spring of next year, I shall then employ my time assiduously in arranging the numbers of this work in a single volume, which will be illustrated by upwards of thirty plates, and embrace an accurate view of the strata containing the fossils hereinafter described.

"CLAIBORNE, ALABAMA, *Nov.* 1, 1833.

"T. A. CONRAD."

In the copy at the Academy of Natural Sciences Conrad has altered the above date with a pen to read Sept. 1st, as it was obviously incongruous that a letter written in November should reach Philadelphia in time to be printed in October of the same year, while it probably took a month for letters to go to Philadelphia from Claiborne, Ala., at that time. On Nov. 26, 1833, Dr. Morton presented to the library the fourth number of the work, as above described, which donation was probably made at the first meeting after the actual publication, not more than two weeks later than the work was issued. (See Am. Journal of Conchology, vol. i, p. 190.) Few copies of the original No. 4 are known to be in existence. There is one at the Academy and one at the Wagner Institute in Philadelphia. Messrs. Aldrich and Meyer possess copies, but there are probably not over six copies in existence. The tradition is that the entire edition, with the exception of a few copies, was purchased by friends of Dr. Lea and destroyed. As Conrad did not have access to Dr. Lea's types at that time, his synonymy in the Appendix to Morton's "Organic Remains" (1834) is largely guess-work and contains many errors and some names, such as *Cerithium sagenula,* which have never been published at all.

After his return from the South, Conrad set at work to redeem the promise made in his name on the cover of No. 4, and issued in March, 1835, a paper covering the same ground

as Nos. 3 and 4 of the first series, but containing new matter, plates, and the specific descriptions rearranged to bring all of one genus together, while remarks and some synonymy are attempted. In most copies of the whole work, so called, the original sheets of Nos. 1 and 2 are bound up with the paper of 1835, but it would seem that the sheet containing pp. i-viii must have run short and been reprinted, since M. de Gregorio has a copy where the dedication is signed "T. A. Conrad," and in which the preface commences on page v instead of iv, as in the original; otherwise it would seem to be an exact reprint of the original. Of the paper of 1835, usually though inexactly called the "reprint," there are two editions. The first edition had grayish green paper covers, a geological map of Alabama, but no title other than that printed on the cover, which reads:

Vol. 1. No. 3. | Fossil shells | of the | Tertiary formation | of | North America. | — | By T. A. Conrad, | Member of the Academy of Natural Sciences | of Philada. and Geological Society | of Pennsylvania. | — | Contents: | Eocene Fossils of Claiborne, | with observations on this formation in the United States, | and a geological map of Alabama. | Republished, with plates, March 1, 1835. |

Then follows [p. 29, beginning but not paginated] "Observations on the Eocene deposits of the United States," pp. 29–36. In the course of the printing, apparently, a correction was made in the text, p. 35, where, line 11 from the bottom, for "Saurian family," "Enaliosauri" has been substituted, but without any other change of type. At the end of the "Observations," p. 36, a paragraph begins: "The following names are adopted for the American Tertiary formations," which are in their order "Newer Pliocene," "Older Pliocene," "Miocene," "Eocene," typical localities of which are mentioned. Then follows the following note:

"It is necessary to observe that all the species of Eocene fossils described in the following pages, unless where credit is given or no reference made, were published before Mr. Lea's "Contributions to Geology." Number 1 was published

August 25, 1833, and No. 2 November 1, 1833. [By No. 1 he here means No. 3, or No. 1 of the "reprint," and by No. 2 the Say-Morton number 4.] They have been reprinted in order to group together the species of each genus. The "Contributions" were published some time after the appearance of No. 3 [here he again means the Say-Morton No. 4; this inaccuracy is absolutely characteristic of Conrad in citations] of the present work. By reference to dates it will be seen that Mr. Lea has been guilty of a plagiarism in giving himself credit for my own observations on the Tertiary formations of the United States. A laborious investigation of these on my part from New Jersey to the Gulf of Mexico has resulted in identifying the Eocene, Miocene, Older and Newer Pliocene, and recent formations of the Union. Mr. Lea, to my knowledge, knew nothing of our coast Formations previous to my observations on that subject, and never travelled out of his closet to make original discoveries, for I have fortunately saved him the trouble." [The charges made by Conrad in the above paragraph were never substantiated, and the expressions used are merely an index of his irritation. It is probably due to them that the pamphlet became so rare, as Mr. Lea and his friends are said to have destroyed as many copies as they could obtain, while the edition was very limited.]

Page 37 begins with the description of species, which continues to page 54; page 55 contains the references to the plates (pl. 15–18) having been misplaced by the printer, while on page 56, which should have had the plate references, the conclusion of the descriptive part terminates with *Fusus salebrosus*. The species and references for the reprint and the original place of publication, respectively, are as follows:

Monoceros armigerus,	p. 37,	tab.	15,	fig.	1.	Orig. p. 44.
" vetustus,	" "	"	15,	"	3.	" " 44.
Melongena alveata,	" "	"	"	"	2.	A. J. S. xxiii.
Conus sauridens,	" 38,	"	"	"	7.	Orig. p. 33.
Rostellaria velata,	" "	"	"	"	4.	" " 31.
Rostellaria laqueata,	" "	"	"	"	5.	" " 41.

Pyrula tricarinata,	p. 38, tab. 15, fig. 6.	Orig. p. 32.
Emarginula arata,	" 39, " " " 8.	" " 44.
Fissurella tenebrosa,	" " " " " 9.	" " 33.
Dentalium thalloides,	" " " " " 10.	" " 34.
Turritella Mortoni,	" 40, " " " 11.	J. Ac. N. S. vi.
" obruta,	" " " " " 12.	Orig. p. 45.
" vetusta,	" " " " " 13.	" " 35.
Bulla galba,	" " " " " 14.	" " 34.
Voluta Sayana,	" 41, " 16, " 1.	" " 29.
" petrosa,	" " " " " 2.	" " 29.
Oliva Alabamensis,	" " " " " 3.	" " 32.
" bombylis,	" 42, " " " 4.	" " 32.
Ancillaria lymnæoides,	" " " " " 6.	" " 44.
" tenera,	" " " " " 5.	— — —
Mitra perexilis,	" " " " " 7.	Orig. p. 46.
" fusoides,	" " " " " 8.	Lea, Contr. Geol.
" pactilis,	" 43, " " " 21.	Orig. p. 46.
Voluta prisca,	" " " " " 9.	" " 45.
Mitra bolaris,	" " " " " 11.	" " 34.
Turbinella prætenuis,	" 44, " 17, " 1.	" " 45.
Caricella, n. g.,	" 44. — — — —	— — —
Cancellaria gemmata,	" " " 16, " 10.	Orig. p. 35.
" alveata,	" " " " " 19.	" " 45.
Marginella larvata,	" 45, " " " 12.	" " 33.
" crassilabra,	" " " " " 13.	" " 33.
" humerosa,	" " " " " 14.	— — —.
" columba,	" " " " " 16.	Lea, Contr. Geol.
" constricta,	" 46, " " " 15.	— — —
Crepidula lirata,	" " " " " 17.	A. J. Sci. xxiii.
" dumosa,	" " " " " 20.	J. Ac. N. Sci. vii.
Infundibulum trochiformis,	" " " " " 18.	Orig. p. 32.
Siliquaria vitis,	" 47, " 17, " 2.	" " 36.
Solarium alveatum.	" " " " " 3.	" " 31.
" elaboratum,	" " " " " 4.	A. J. Sci. xxiii.
" exacuum,	" 48, " " " 5.	Orig. p. 44.
" stalagmium,	" " " " " 6.	" " 44.
" funginum,	" " " " " 7.	" " 44.
" amœnum,	" 49, " " " 8.	" " 44.
" patulum,	" " " " " 9.	" " 49.
" cancellatum,	" " " " " 11.	A. J. Sci. xxiii.
" tricostatum,	" 50, " " " 10.	Lea, pl. 4, f. 111.
" lineatum,	" " " " " 12.	Orig. p. 44.
Pleurotoma alternata,	" " " " " 13.	" " 46.
" tabulata,	" " " " " 14.	" " 46.
" proruta,	" 51, " " " 15.	Fusus, Morton Cat.
" nupera,	" " " " " 16.	Orig. p. 46.

Pleurotoma conoides,	p. 51,	tab. 17,	fig.	17.	—	—	—
" subequalis,	" "	"	"	" 18.	—	—	—
" elaborata,	" 52,	"	"	" 19.	Orig.	p.	46.
" depygis,	" "	"	"	" 20.	"	"	46.
" acutirostra,	" "	"	"	" 21.	—	—	—
" gemmata,	" "	"	"	" 22.	—	—	—
" callifera,	" "	—	—	— —	—	—	—
Fusus trabeatus,	" 53,	"	18,	" 1.	Orig.	p.	29.
" inauratus,	" "	"	"	" 2.	"	"	29.
" papillatus,	" "	"	"	" 3.	"	"	29.
" limulus,	" "	"	"	" 4.	"	"	43.
" perlatus,	" 54,	"	"	" 5.	"	"	45.
" thoracicus,	" "	"	"	" 6.	"	"	30.
" protextus,	" "	"	"	" 7.	"	"	43.
" raphanoides,	" "	"	"	" 8.	J. Ac. N. Sci. vii.		
" irrasus,	" "	"	"	" 10.	" " " "		
" bellus,	" 56,	"	"	" 11.	Orig.	p.	43.
" thalloides,	" "	"	"	" 12.	"	"	43.
" salebrosus,	" "	"	"	" 13.	J. Ac. N. Sci. vii.		
Ranella Maclurei,	" 55,	"	"	" 9.	Name and fig. only.		

After page 56 follow plates 15–18. The back leaf of the cover is blank. Nothing in continuation was ever published.

This paper is extremely scarce also, though not so scarce as the Say and Morton number. Prof. E. A. Smith, Mr. T. H. Aldrich, Harvard College, and the writer possess copies of it. It was followed by a second edition at some time between 1842 and 1845, which is for the most part identical, if not, indeed, partly made up of sheets printed for the first issue. In the introduction there are no essential changes, but on page 36 we find under the caption, "The following terms are adopted for the American Tertiary formations," a new set of terms, "Upper Tertiary or Newer Pliocene," "Medial Tertiary or Newer Pliocene," and "Lower Tertiary or Eocene," Miocene being omitted.

Then follows this note about matters of priority, the objectionable references to Mr. Lea having been expunged; the words in brackets are added for clearness:

"It is necessary to observe that all the species of Eocene fossils described in the following pages, unless where credit

is given or no reference made, were published before Mr. Lea's 'Contributions to Geology.' [Original] No. 3 was published August, 1833, and [Say & Morton] No. 4, October, 1833. They have been reprinted in order to group together the species of each genus."

The space occupied in the first reprint by the attack on Lea is now filled with a paragraph headed "Corrigenda," which refers to tab. 2, fig. 2, and says of the species there represented, "This was described as *P*[ectunculus]. *pulvinatus,* but it is now ascertained to be very distinct, and it is proposed to name it *P. lentiformis,*" etc.

This edition, being less obnoxious to criticism by the friends of Mr. Lea, is the one most commonly found in copies of the work. The preceding references will enable any one to make the correct reference and date for all Conrad's contested Eocene species and to judge of the disposition which should be made of them.

It may be noted that the plates for this and other works of Conrad were drawn and sometimes even put on the stone by him, one or two stones being used, and after the edition needed had been printed the stone was cleaned off and another drawing made. Mr. Conrad being in modest circumstances, could not afford to print large editions, and what he published in this way probably never paid for his expenses, which will explain why so few copies were published and why it was comparatively easy to suppress the major part of the editions printed.

Several years later Mr. Conrad projected his "Medial Tertiary" monograph, also a rare and unfinished work, of which it seems as if a collation would be useful to students.

It appeared in covers, of which the first leaf comprised a lithographed title with a figure of *Ecphora,* drawn by Conrad himself, with a blank space intended to be filled by the number of the part [written in with a pen], and another for the date [also written]. These covers are printed on colored paper, brownish, blue, or yellow, and the same cover was used for successive parts, only differing by the manuscript

entries. Sometimes when there were a few diagnoses over enough to fill a "signature" of 8 or 16 pages, the excess was printed on the cover, and sometimes the cover of one and the same part was *twice* surcharged; in this way some covers have fewer diagnoses than are to be found on other presumably later, copies. Mr. Conrad's extraordinary and habitual carelessness, or want of memory, which grew upon him, especially in later years, to such an extent that he finally decided to attempt no more work, was a marked factor in inducing variations. It would seem from the differences observed in the various copies I have examined that the sheets were kept on hand and made up when called for from time to time by subscribers. The edition of a plate did not always hold out, and in nearly every copy there is at least one plate which is not the original but a substitute. For some of the plates Mr. Conrad used the copper plates engraved by Lesueur for the first series of the Journal of the Academy of Natural Sciences, simply stopping out the Academy's heading on his prints. Sometimes in copying a plate on the lithographic stone to supply a deficiency the new copy would differ in shading or figures would be added which were not on the first edition of it. There are frequent cases where the same plate is found in two states with two different printers' names at the bottom, though the copies are otherwise exactly alike. In several cases the second edition of a plate is wrongly numbered, so that in the copy containing it there will be two plates with the same number and one apparently deficient. The plates, if correctly numbered and all present, should number for the four parts 1–32 and 34–49; there never was any plate 33. The subscribers in binding did not always save the covers. It will therefore not surprise the reader to learn that the utmost search has not revealed an absolutely perfect and complete copy of the work nor any two exactly similar copies of it. Nevertheless, the differences are not of very great importance. Part 1 was marked by Conrad as issued January, 1838. It came out with a plain cover, so far as

the three pages after the title are concerned, but the cover was surcharged with diagnoses in April, 1839, and with another lot December, 1839. Four more were printed without a date (about Sept., 1841, probably) on the fourth page of some copies, and other copies have the second and third pages of cover blank and only the four diagnoses on the fourth page. This last form, I believe, belongs, strictly speaking, only to number 2 in its last state (see Medial Tert., p. 82), but is sometimes found bound up with No. 1.

The collation is as follows, the first page of cover:

No. 1. | Fossils | of the | Medial Tertiary | of the | United States. | [Figure of Ecphora.] | By T. A. Conrad. | Jan., 1838 [in Ms.] | Philadelphia: | Judah Dobson, 108 Chestnut St. |

Title page follows:

Fossils | of the | Tertiary formations | of the | United States. | Illustrated by figures. Drawn from nature. | By T. A. Conrad, | Paleontologist of the State of New York; Member of the Academy of | Natural Sciences of Philadelphia; of the Imperial Society | of Natural History of Moscow, Etc., Etc. | — | Philadelphia: | J. Dobson, 108 Chestnut Street. | E. G. Dorsey, Printer. | 1838. |

On reverse of the title is copyright notice dated 1838. On the following [unpaginated] page is an unsigned dedication of 14 lines to Samuel George Morton, M. D. On page v is "Introduction," followed by an account of the "Medial Tertiary formation," which closes (p. xvi) with a list of the recent species which occur in it, and of fossils in it common to Europe and Asia, and ends with a "Table of the Tertiary formations." The pagination then changes, and on page 1, headed "Fossils of the Medial Tertiary formation, &c.," the description of species begins and extends to p. 32. Then follow plates 1–17. The following species are described on the third page of the cover, printed April 16, 1839: *Serpula virginica, Oliva idonea, Fulgur maximus, Cardium virginianum, C. acutilaqueatum, Venus latisulcata, Cytherea staminea,*

Myoconcha incurva, and *Pectunculus carolinianus*. In March, 1840, on the second page of the cover was added *Arca elevata* from the Choptank river, Md. This cover in this state will be referred to as cover A; it was afterward used for some copies of Part 2.

As regards the plates in Part 1, for Plate 3 in several copies is substituted a copper plate with the same species from the Journal Acad. Nat. Sci. In Gabb's copy plate 7 is similarly replaced. Plate 9 has two states, one printed by Lehman and Duval and the other by Sinclair; the same is true of plate 10. These two are erroneously numbered xxii and xxiii and were perhaps originally drawn for the unpublished part of the Eocene volume. Plate 11 is sometimes replaced by Lesueur's copper plate from the Journal.

No. 2 is marked May 7, 1840, in Conrad's handwriting. It has in my copy the same cover which I have called A, except that on the fourth page of cover (about September, 1841, as nearly as I can discover) diagnoses were printed of *Pholas arcuata*, *Fulgur coronatus*, *Fulgur tuberculatus*, and *Fulgur fusiformis*.

The text extends from p. 33 to p. 56, inclusive, and the plates from 18 to 29, inclusive. Other copies have pages 2 and 3 of the cover plain.

Plate 19 has two states; the earlier has 8 figures, the view of figure 7 being double. The second state has plate number 18 by error, and has fifteen figures which are not numbered with the same numbers as in the first state. On the latter fig. 6 was *Amphidesma subreflexa*, but it is *Lucina* (divaricata =) *Conradi* Orb. on the second. The two figures marked 7 on the first represent *Amphidesma carinata*, while on the second 7 indicates *Lucina* (squamosa =) *speciosa* Rogers. *A. carinata* becomes figure 11 and *A. subreflexa* fig. 12 in the second state of the plate and figures of the above-cited Lucinas and of *L. crenulata* (which also appear on the original plate 20), *Nucula obliqua* and *N. acuta* (which also appear on plate 30). Plate 21 is numbered 23 by error. Plate 24

has two states, one in which the inside of the valves is shaded, while the other has it white.

No. 3 has a new cover, marked "Fossils of the Miocene formation of the United States;" otherwise the first side is like that of the cover of Nos. 1 and 2. On my copy in Conrad's handwriting is the date, "Jan., 1845." The text includes pages 57–80, and the plates 30–32 and 34–44, no plate 33 having existed. I have seen no variants of the plates of this number. Except the first page, the cover is blank; on the front of it, after the word "Miocene," Conrad has written in the words "or Medial Tertiary," in my copy. On page 77 is a reference to H. C. Lea's *Teredo fistula*, which was not described until 1845, but the reference is not complete and was probably taken from a specimen donated to the Academy by Mr. Lea, who published a list of names, without descriptions, in the Proceedings of the Am. Phil. Society before his paper in the Transactions was printed. No. 4 comprised pp. 81–86, descriptions of species, and 87–89, index, followed by plates 45–49. The last leaf of the cover was blank on both sides, as well as the second page of it. On Gabb's copy, under date of August 10, 1861, he has written the following remarks:

"From page 81 to end of the text was first published in 1861, about March or April, I believe; perhaps later, certainly not earlier.—W. M. G."

The following list of species published in the "Medial Tertiary" may be of use to students. The abbreviations used to indicate places of prior publication or other references are, J. Ac., for Journal of the Academy of Natural Sciences; Proc. Ac., Proceedings of the Academy; Eo. fos., for Conrad's Fossils of the Tertiary formation N. Am., 1832–'42; S. J., for Silliman's American Journal of Science and Arts; Am. Ph., for Transactions American Philosophical Society; Am. Con., for Say's American Conchology; Min. Con., Sowerby's Mineral Conchology; Mar. Con., Conrad's American Marine Conchology, etc. The references have

been verified; many of them were wrong in the original. It may be noted that in the imperfect index printed on pp. 86–'9, in 1861, by Mr. Conrad, several generic names were changed, *Axinea* being substituted for *Pectunculus*, etc. These changes may be found in Mr. Conrad's "Catalogue of the Miocene shells of the Atlantic Slope" (Proc. Acad. Nat. Sci., Phila., for 1862, pp. 559–582; covering June–December, 1862, and distributed by the author in March, 1863), and it has not been thought necessary to swell the index by including them.

Amphidesma æquata, p. 65, pl. 36, f. 5; Proc. Ac. i, 307, Oct. 1843.
Amphidesma carinata, p. 37, pl. 19 [1st ed.], f. 7; [2d ed.] f. 11; J. Ac. vi, 229, 1830, pl. ix, f. 23 (*Abra*, 1862).
Amphidesma constricta, p. 76, pl. 43, f. 10; S. J. xli, 347, Oct. 1841.
Amphidesma equalis Say, p. 76, pl. 43, f. 9; J. Ac. ii, 307, 1822.
Amphidesma nuculoides, p. 73, pl. 41, f. 6; S. J. xli, 347, Oct. 1841.
Amphidesma protexta, p. 73, pl. 41, f. 7; S. J. xli, 347, Oct. 1841.
Amphidesma subovata Say, p. 36 (no fig.; *Abra s.* Conr. 1862); J. Ac. iv, 152, pl. x, f. 6, 1824.
Amphidesma subreflexa, p. 37, pl. 19, [1st ed.] f. 6; [2d ed.] f. 12; J. Ac. vii, 133, 1834, (*Abra*, Conr. 1862).
Anguinella, see *Serpula*.
Anguinella virginiana, p. 77, pl. 44, f. 4, 1845 (= Serpula virginica Conr. 1839.)
Anomia ephippium Lam. var., p. 75, pl. 43, f. 4.
Anomia Ruffini, p. 74, pl. 42, f. 6, Jan. 1845.
Arca æquicostata, p. 60, pl. 31, f. 6, Jan. 1845.
Arca arata Say, p. 58, pl. 30, f. 6; J. Ac. iv, p. 137, pl. 10, f. 1, 1824.
Arca brevidesma, p. 62, pl. 32, f. 5, Jan. 1845.
Arca buccula, p. 60, pl. 31, f. 4, Jan. 1845.
Arca cælata, p. 61, pl. 32, f. 2, Jan. 1845.
Arca callipleura, p. 54, pl. 29, f. 2, May 1840.
Arca centenaria Say, p. 55, pl. 29, f. 4; J. Ac. iv, 138, Dec. 1824; Eo. fos., p. 16.
Arca elevata, p. 2, cover of No. 1, Mar. 1840.
Arca idonea, p. 55, pl. 29, f. 3; + *A. stillicidium*, Eo. fos., p. 15, pl. 1, f. 3, 5, Oct. 1832.
Arca improcera, p. 60, pl. 31, f. 5, Jan. 1845.
Arca incile Say, p. 56, pl. 29, f. 5; J. Ac. iv, 139, Dec. 1824; Eo. fos., p. 16.
Arca limula, p. 60, pl. 31, f. 3; Eo. fos., p. 15, pl. 1, f. 1, Oct. 1832.
Arca lineolata, p. 61, pl. 32, f. 3, Jan. 1845.
Arca plicatura, p. 61, pl. 32, f. 4, Jan. 1845.
Arca propatula, p. 61, pl. 32, f. 1; Proc. Ac. i, p. 323, Dec. 1843.

Arca protracta Rogers, p. 58, pl. 30, f. 5; Am. Ph. v, pl. 26, f. 5, 1837.
Arca scalaris, p. 59, pl. 31, f. 1; Proc. Ac. i, 324, Dec. 1843.
Arca subrostrata, p. 58, pl. 30, f. 7; Proc. Ac. i, p. 30, July 1841.
Arca subsinuata, p. 62, pl. 32, f. 6, Jan. 1845.
Arca triquetra, p. 59, pl. 31, f. 2; Proc. Ac. i, p. 305, Oct. 1843.
Artemis acetabulum, p. 29, pl. 16, f. 1; Eo. fos., p. 20, Oct. 1832.
Artemis elegans, p. 67, pl. 38, f. 1; Proc. Ac. i, 325, Dec. 1843.
Astarte abbreviata, p. 77, pl. 43, f. 12; S. J. xli, p. 347, pl. 2, fig. 17, Oct. 1841.
Astarte arata, p. 42, pl. 20, f. 8, May 1840.
Astarte Coheni, p. 43, pl 21, f. 5, May, 1840.
Astarte concentrica, p. 44, pl. 21, f. 6; J. Ac. vii, 133, 1834.
Astarte cuneiformis, p. 42, pl. 20, f. 9, May 1840.
Astarte exaltata, p. 66, pl. 37, f. 6; Proc. Ac. i, 29, July 1841 (= *A. vicina* Say).
Astarte lunulata, p. 44, pl. 21, f. 8; J. Ac. vii, 133, 1834.
Astarte obruta, p. 43, pl. 21, f. 2; J. Ac. vii, 150, 1834.
Astarte perplana, p. 43, pl. 21, f. 3, May 1840.
Astarte radians, p. 77, pl. 43, f. 13; S. J. xli, pl. 2, f. 16, Oct. 1841.
Astarte symmetrica, p. 44, pl. 21, f. 7; J. Ac. vii, 134, 1834.
Astarte undulata Say, p. 41, pl. 20, f. 7, pl. 21, f. 4, (var.); J. Ac. iv, 150, 1824.
Astarte varians, p. 67, pl. 37, f. 7; Proc. Ac. i, 29, July 1841.
Astarte vicina Say, p. 41; J. Ac. iv, 151, pl. ix, f. 6, 1824.
Balanus proteus, p. 77, pl. 44, f. 1; J. Ac. vii, 134, 1834.
Busycon, see *Fulgur*.
Busycon coronatum, p. 82, pl. 46, f. 1, see *Fulgur coronatus*.
Busycon contrarium, p. 81, pl. 45, f. 11; S. J. xxxiv, 387, 1840.
Busycon excavatum, p. 82, pl. 45, f. 12; S. J. xxxix, 387, 1840.
Busycon fusiforme, p. 82, pl. 46, f. 3; see *Fulgur f.*
Busycon maximum, p. 83, pl. 47; see *Fulgur maximus*.
Busycon rugosum, p. 83, pl. 46, f. 4; Proc. Ac. i, 307, Oct. 1843.
Busycon tuberculatum, p. 82, pl. 46, f. 2; see *Fulgur t.*
Byssoarca marylandica, p. 54, pl. 29, f. 1, May 1840.
Calyptræa pileolus H. C. Lea, p. 79; Am. Ph. ix, p. 248, pl. 35, f. 38, 1845.
Cardita granulata Say, p. 12, pl. 7, f. 1; J. Ac. iv, 142, 1824.
Cardita perplana Conr., p. 77; S. J. xli, p. 347, pl. 2, f. 16, Oct. 1841.
Cardita tridentata Say, p. 76, pl. 43, f. 11; J. Ac. v, 216, Feb. 1826.
Carditamera n. g., p. 11, Jan. 1838.
Carditamera arata, pp. 10,* 11, pl. 6, f. 2; Eo. fos., p. 20, Oct. 1832.
Carditamera carinata, p. 65, pl. 37, f. 1; Proc. Ac. i, 305, Oct. 1843.
Carditamera floridana, p. 12, (recent) Jan. 1838.
Carditamera protracta, p. 65, pl. 37, f. 2; Proc. Ac. i, 305, Oct. 1843.
Cardium acutilaqueatum, p. 3, cover No. 1, Apr. 1839; p. 34, pl. 18, f. 2.
Cardium craticuloides, p. 66, pl. 37, f. 3, Jan. 1845.
Cardium ingens Wagner, p. 33, see *C. virginianum*.

Cardium laqueatum, p. 31, pl. 17, f. 1; J. Ac. vi, 258, Mar. 1831.
Cardium leptopleura, p. 66, pl. 37, f. 5; Proc. Ac. i, 29, July 1841.
Cardium quadrans Rogers, Dec. 1839, see *C. virginianum.*
Cardium sublineatum, p. 66, pl. 37, f. 4; S. J. xli, 347, Oct. 1841.
Cardium virginianum, cover of No. 1, pp. 32,* 33, pl. 18, f. 1, Apr. 1839.
Chama congregata, p. 32, pl. 17, f. 2; S. J. xxiii, p. 341, July 1833.
Chama corticosa, p. 32, pl. 17, f. 3; S. J. xxiii, p. 341, July 1833.
Corbula cuneata Say, p. 5, pl. 3, f. 2; J. Ac. iv, 152, 1824.
Corbula elevata, p. 7, pl. 4, f. 3, Jan. 1838.
Corbula idonea, p. 6, pl. 10, f. 6; S. J. xxiii, 341, July 1833.
Corbula inæquale Say, p. 6, pl. 3, f. 3; J. Ac. iv, 153, 1824.
Crassatella marylandica, p. 21, pl. 12, f. 1; Eo. fos., p. 22, Dec. 1832.
Crassatella melina, p. 22, pl. 12, f. 2; Eo. fos., p. 23, Dec. 1832.
Crassatella turgidula, p. 69, pl. 39, f. 7; Proc. Ac. i, 307, Oct. 1843.
Crassatella undulata Say, p. 21, pl. 11, f. 2; J. Ac. iv, p. 142, 1824.
Crypta cymbæformis, p. 81, pl. 45, f. 7; Proc. Ac. ii, 173, Dec. 1844.
Crypta densata, p. 81, pl. 45, f. 9; Proc. Ac. i, 311, Oct. 1843.
Crypta fornicata Lam., p. 81; Say, J. Ac. ii, 225, 1821.
Crypta spinosa, p. 81, pl. 45, f. 3; Proc. Ac. i, 307, Oct. 1843.
Crucibulum, see *Dispotæa.*
Cultellus caribbæus Conr., p. 75, pl. 43, f. 1 (not of Lam.).
Cumingia tellinoides, p. 28, pl. 15, f. 4; = Mactra, J. Ac. vi, 258, Mar. 1831.
Cyrena densata, p. 68, pl. 39, f. 2; Proc. Ac. i, 324, Dec. 1843.
Cytherea albaria Say, p. 13, pl. 8, f. 2; Am. Con., pl. 59, Apr. 1834.
Cytherea marylandica, p. 15, pl. 9, f. 1; S. J. xxiii, 343, July 1833.
Cytherea (Circe) metastriata, p. 14, pl. 8, f. 5, Jan. 1838.
Cytherea obovata, p. 14, pl. 8, f. 4; J. Ac. vii, 132, 1834.
Cytherea reposta, p. 15, pl. 9, f. 2; J. Ac. vii, 132, 1834.
Cytherea Sayana, p. 13, pl. 7, f. 3; J. Ac. iv, 149, 1824.
Cytherea staminea, p. 3, cover of No. 1, Apr. 16, 1839, pl. 21, f. 1.
Cytherea subnasuta, p. 72, pl. 41, f. 3; Proc. Ac. i, 28, July 1841.
Dentalium dentale (Con. non Lin.), p. 78, pl. 44, f. 9; = *D. attenuatum* Say.
Dentalium thallus, p. 78, pl. 44, f. 5; J. Ac. vii, 142, 1834.
Diplodonta, see *Mysia.*
Diplodonta elevata, Con. Pro. Ac. ix, 166, see *Mysia americana.*
Dispotæa constricta, p. 80, pl. 45, f. 4; Bull. Nat. Inst. i, p. 194, 1842.
Dispotæa costata Say, p. 79, pl. 45, f. 2; J. Ac. iv, 132, 1824.
Dispotæa dumosa, p. 80; S. J. xli, p. 348, pl. 2, f. 9, Oct. 1841.
Dispotæa grandis Say, p. 79; J. Ac. iv, p. 132, pl. vii, f. 6, 1824.
Dispotæa multilineata, p. 80; S. J. xli, p. 348, pl. 2, f. 8, Oct. 1841.
Dispotæa ramosa, p. 79, pl. 45, f. 3, Jan. 1845.
Dosinia, see *Artemis.*
Ecphora quadricostata Say, p. 83, pl. 48, f. 2; J. Ac. iv, 127, 1824.
Egesta n. subg. Conr., p. 70, Jan. 1845.
Erycinella n. g. Conr., p. 74, Jan. 1845.
Erycinella ovalis, p. 74, pl. 42, f. 5, Jan. 1845.

Fasciolaria rhomboidea Rogers, p. 84, pl. 48, f. 3; Am. Ph. vi, 376, Dec. 1839.
Fasciolaria sulcosa, p. 86, pl. 49, f. 7; J. Ac. vi, 220, 1829.
Fissurella alticosta, p. 78, pl. 44, f. 7; J. Ac. vii, 142, 1834.
Fissurella Griscomi, p. 78, pl. 44, f. 8, Jan. 1845.
Fissurella marylandica, p. 79, pl. 45, f. 1; Proc. Ac. i, 31, July 1841.
Fissurella nassula, p. 78, pl. 44, f. 6, Jan. 1845.
Fissurella redimicula Say, p. 78; J. Ac. iv, p. 132, pl. 8, f. 1, 1824.
Fulgur, see *Busycon*.
Fulgur coronatus, p. 3, cover of No. 2; p. 82, pl. 46, f. 1 (Sept. 1841?)
Fulgur fusiformis, p. 3, cover of No. 2; p. 82, pl. 46, f. 3, 1841.
Fulgur maximus, p. 3 of cover No. 1, Apr. 16, 1839; p. 83, pl. 47, 1861.
Fulgur tuberculatus, p. 3, cover of No. 2; p. 82, pl. 46, f. 2 (Sept. 1841?)
Fusus devexus, p. 86, pl. 49, f. 8; Proc. Ac. i, 309, Oct. 1843.
Fusus exilis, p. 85, pl. 49, f. 1, 4; Eo. fos., p. 17, 1832.
Fusus filicatus, p. 85, pl. 49, f. 2; Proc. Ac. i, 308, Oct. 1843.
Fusus migrans, p. 85, pl. 49, f. 6; Proc. Ac. i, 309, Oct. 1843.
Fusus parilis, p. 85, pl. 49, f. 5; Eo. fos., p. 19, 1832.
Fusus tetricus, p. 84, pl. 48, f. 4; Eo. fos., p. 18, pl. 3, f. 6, 1832.
Fusus trossulus, p. 84, pl. 48, f. 6; Eo. fos., p. 18, 1832.
Fusus strumosus, p. 85, pl. 49, f. 3; Eo. fos., p. 18, 1832.
Fusus subrusticus Orb., p. 84, pl. 48, f. 5; Prodr. Pal. iii, 69 = *F. rusticus* Conr., Eo. fos., p. 18, pl. 4, f. 1, 1832.
Gnathodon (Gray Ms.), n. g., p. 23, Jan. 1838.
Gnathodon Grayi, p. 23, pl. 13, f. 1; =*Mactra clathrodonta* Conr., S. J. xxiii, p. 340, July 1833.
Gnathodon minor, p. 69, pl. 39, f. 6; S. J. xli, pl. 2, f. 14, Oct. 1841.
Infundibulum centralis, p. 80, pl. 45, f. 5; S. J. xli, 348, Oct. 1841.
Infundibulum perarmatum, p. 80, pl. 45, f. 6; Proc. Ac. i, 31, July 1841.
Isocardia Markoei, p. 70, pl. 40, f. 2; Bull. Nat. Inst. i, p. 193, 1841.
Isocardia rustica Sow., p. 20, pl. 11, f. 1; Min. Con. ii, p. 217, pl. 196, 1818.
Leda, see *Nucula*.
Lepton mactroides, p. 19, pl. 10, f. 5; J. Ac. vii, 151, 1834.
Lima papyria, p. 76, pl. 43, f. 7; Proc. Ac. i, 30, July 1841.
Loripes elevata, p. 73, pl. 41, f. 8; Proc. Ac. i, 325, Dec. 1843.
Lucina acclinis, see *Mysia americana*.
Lucina anodonta Say, p. 39, pl. 20, f. 4; J. Ac. iv, 146, 1824.
Lucina contracta Say, p. 40, pl. 20, f. 5; J. Ac. iv, 145, 1824.
Lucina crenulata, p. 39, pl. 19 [2d ed.], f. 8; pl. 20, f. 2, May 1840.
Lucina cribraria Say, p. 6*, pl. 3, f. 1; J. Ac. iv, 147, Dec. 1824.
Lucina divaricata [Conr. non Lin. = Conradi Orb.], p. 39, pl. 19 [2d ed.], f. 6; pl. 20, f. 3, May 1840.
Lucina Foremani, p. 71, pl. 40, f. 4; Proc. Ac. i, 29, July, 1841.
Lucina multistriata, p. 71, pl. 40, f. 6; Proc. Ac. i, 307, Oct. 1843.
Lucina radians, p. 70, pl. 40, f. 3; S. J. xli, 347, Oct. 1841.
Lucina squamosa Lam. (= *speciosa* Rogers, Am. Ph. v, 333, 1836), p. 38, pl. 19 [2d ed.], f. 7; pl. 20, f. 1; An. s. Vert. v, 542, 1818.

Lucina trisulcata, p. 71, pl. 40, f. 5; S. J. xli, 346, Oct. 1841.
Lucina undula, p. 71, pl. 41, f. 1, Jan. 1845.
Mactra congesta, p. 27, pl. 15, f. 2; S. J. xxiii, 340 (+ *M.* [*Spisula*] *trigonalis* Conr. Ms.), July, 1833.
Mactra crassidens, p. 69, pl. 39, f. 5; S. J. xli, p. 347, pl. 2, f. 11, Oct. 1841.
Mactra delumbis, p. 27, pl. 15, f. 1; Eo. fos., p. 26, Dec. 1832.
Mactra fragosa, p. 26, pl. 14, f. 2 (= *Mesodesma confraga*, S. J. xxiii, 340). July 1833.
Mactra incrassata, p. 24, pl. 13, f. 2 (= *Mesodesma* Pro. Ac. 1862, 574).
Mactra modicella, p. 25, pl. 13, f. 3; S. J. xxiii, 340, July 1833.
Mactra ponderosa, p. 25, pl. 14, f. 1; J. Ac. vi, 228, 1830.
Mactra subcuneata, p. 28, pl. 15, f. 3, Jan. 1838.
Mactra subparilis, p. 69, pl. 39, f. 4; S. J. xli, p. 346, pl. 2, f. 12, Oct. 1841,
Mactra triquetra, p. 69, pl. 39, f. 3; Proc. Ac. i, 324, Dec. 1843.
Marginella americana, p. 86, pl. 49, f. 12. No description. = *M. columba* Lea.
Marginella denticulata, p. 86, pl. 49, f. 10, J. Ac. vi, 225, 1830.
Marginella eburneola, p. 86 [not pl. 49, fig. 11]; J. Ac. vii, 141, 1834.
Marginella limatula, p. 86, pl. 49, f. 11 [9 by error in text]; J. Ac. vii, 140, 1834.
Marginella oviformis, pl. 49, f. 9, Am. Journ. Conch., p. 66, pl. 5, f. 3, 1868. No name appears for this figure in the Medial Tert. text where it was erroneously referred to *M. limatula.*
Modiola Ducatellii, p. 53, pl. 28, f. 2, May 1840.
Murex, see *Typhis.*
Mya corpulenta, p. 68, pl. 39, f. 1, Jan. 1845.
Mya producta, p. 1, pl. 1, f. 1, Jan. 1838.
Myalina subovata, p. 65, pl. 36, f. 5, Jan. 1845.
Myoconcha incurva, p. 3, cover of No. 1, 1839; p. 52, No. 2, May 1840, pl. 28, f. 1.
Mysia americana Defr., p. 30, pl. 16, f. 2; Dict. Sci. Nat. art. *Lucina*, 1823.
Mytilus incrassatus, p. 74, pl. 42, f. 3; S. J. xli, 347, Oct. 1841.
Nucula acuta, p. 57, pl. 30, f. 2; Mar. Conch., p. 32, pl. vi, f. 3, May 1832.
Nucula concentrica Say, p. 57, pl. 30, f. 3; J. Ac. iv, pl. 10, f. 6, 1824.
Nucula liciata, p. 64, pl. 36, f. 3; Proc. Ac. i, 305, Oct. 1841.
Nucula limatula Say, p. 57, pl. 30, f. 4; Am. Con., pl. 12, April 1831. (? = *N. lævis* Say.)
Nucula obliqua Say (non Lam.), p. 57, pl. 19, f. 10; pl. 30, f. 1; S. J. ii, p. 40, 1820.
Oliva idonea, p. 3 of cover, No. 1, Apr. 16, 1839.
Orbicula lugubris, p. 75, pl. 43, f. 2; J. Ac. vii, 143, 1834.
Orbicula multilineata, p. 75, pl. 43, f. 3, Jan. 1845.
Ostrea disparilis, p. 51, pl. 26, May 1840.
Ostrea percrassa, p. 50, pl. 25, f. 1, May 1840.
Ostrea sculpturata, p. 50, pl. 25, f. 3, May 1840; = *O. virginiana* Conr., Eo. fos., p. 28, pl. xiv, f. 2.

Ostrea subfalcata, p. 50, pl. 25, f. 2, May 1840.
Pandora arenosa, p. 2, pl. 1, f. 3; J. Ac. vii, 130, 1834.
Pandora crassidens, p. 2, pl. 1, f. 2. Jan. 1838.
Panopæa americana, p. 4, pl. 2, f. 1, Jan. 1838.
Panopæa porrecta, p. 71, pl. 41, f. 2, Jan. 1845.
Panopæa reflexa Say, p. 5, pl. 3, f. 4; J. Ac. iv, 153, 1824.
Paramya, see *Myalina*.
Pecten biformis, p. 73, pl. 42, f. 1; Proc. Ac. i, 306, Oct. 1843.
Pecten Clintonius Say, p. 47, pl. 23, f. 1; J. Ac. iv, 135, 1824.
Pecten decemnarius, p. 49, pl. 24, f. 2; J. Ac. vii, 151, 1834.
Pecten dispalatus, p. 74, pl. 42, f. 3, Jan. 1845.
Pecten eboreus, p. 48, pl. 23, f. 2, pl. 24, f. 3; S. J. xxiii, 341, 1843.
Pecten Jeffersonius Say, p. 46, pl. 22, f. 1; J. Ac. iv, 133, 1824.
Pecten Madisonius Say, p. 48, pl. 24, f. 1; J. Ac. iv, 134, 1824.
Pecten Rogersi, p. 45, pl. 21, f. 9; J. Ac. vii, 151, 1834.
Pecten septemnarius Say, p. 47, pl. 22, f. 2; J. Ac. iv, 136, 1824.
Pecten tricenarius, p. 74, pl. 42, f. 2; Proc. Ac. i, 306, Oct. 1843.
Pecten virginianus, p. 46, pl. 21, f. 10, May 1840.
Pectunculus aratus, p. 62, pl. 34, f. 2; S. J. xli, 346, Oct. 1841.
Pectunculus carolinensis, p. 63, pl. 35, f. 2; S. J. xli, 346, Oct. 1841.
Pectunculus carolinianus, p. 3, cover of No. 1, Apr. 1839.
Pectunculus lentiformis, p. 64, pl. 36, f. 1; Eo. fos., 2d ed., p. 36, 1841.
Pectunculus parilis, p. 64, pl. 36, f. 2; Proc. Ac. i, 306, Oct. 1841.
Pectunculus passus, p. 64, pl. 35, f. 3, Jan. 1845.
Pectunculus quinquerugatus, p. 63, pl. 34, f. 3; S. J. xli, 346, Oct. 1841.
Pectunculus subovatus Say, p. 62, pl. 34, f. 1; J. Ac. iv, 140, 1824.
Pectunculus tricenarius, p. 63, pl. 35, f. 1, Jan. 1845.
Pectunculus tumulus, p. 72, pl. 41, f. 4, Jan. 1845.
Periploma antiqua, p. 16, pl. 8, f. 3; J. Ac. vii, 130, 1834.
Perna maxillata Lam., p. 51, pl. 27, May 1, 1840, = *P. torta* Say.
Petricola centenaria, p. 17, pl. 10, f. 1; S. J. xxiii, 341, July 1833.
Pholadomya abrupta, p. 3, pl. 1, f. 4; Eo. fos. 26, Dec. 1832.
Pholas acuminata, p. 77, pl. 44, f. 2, Jan. 1845; = *P. arcuata*, 1841.
Pholas arcuata, p. 3, cover of No. 2, pl. 44, f. 2 (Sept. 1841?) = *P. acuminata* Con.
Plicatula marginata Say, p. 75, pl. 43, f. 5; J. Ac. iv, 136, pl. ix, f. 4. Dec. 1824.
Plicatula densata, p. 75, pl. 43, f. 6, Jan. 1845.
Saxicava bilineata, p. 18, pl. 10, f. 4, Jan. 1838.
Saxicava pectorosa, p. 18, pl. 10, f. 3; J. Ac. vii, 130, 1834.
Serpula virginica, p. 3 of cover No. 1, Apr. 16, 1839; p. 77, pl. 44, f. 4, 1845.
Solen ensiformis, p. 76, pl. 43, f. 8, Jan. 1845.
Sphærella n. g., p. 17, Jan. 1838.
Sphærella subvexa, p. 18, pl. 10, f. 2, Jan. 1838.
Tellina arctata, p. 72, pl. 41, f. 5; Proc. Ac. i, 306, Oct. 1843.
Tellina biplicata, p. 36, pl. 19, f. 4; J. Ac. vii, 152, 1834.

Tellina declivis, p. 35, pl. 19, f. 1 ; J. Ac. vii, 131, 1834.
Tellina egena, p. 35, pl. 19, f. 2 ; J. A. vii, 131, 1834.
Tellina lenis, p. 72, pl. 41, f. 9 ; Proc. Ac. i, 306, Oct. 1843.
Tellina lusoria Say, p. 35, pl. 19, f. 3 ; J. Ac. ii, 304, 1822.
Tellina producta, p. 36, pl. 19, f. 5, May 1840.
Teredo fistula, H. C. Lea, p. 77, pl. 44, f. 3 ; Am. Ph. ix, 234, 1845.
Typhis acuticostata, p. 83, pl. 48, f. 1 ; J. Ac. vi, 217, 1830.
Venericardia, see *Astarte*.
Venus alveata, p. 9, pl. 5, f. 2 ; J. Ac. vi, 264, Mar. 1831.
Venus capax, p. 68, pl. 38, f. 4 ; Proc. Ac. i, 324, Dec. 1841.
Venus cortinaria Rogers, p. 11, pl. 8, f. 1 ; Am. Ph. v, 333, 1835.
Venus cribraria, p. 67, pl. 38, f. 2 ; Proc. Ac. i, 311, Oct. 1843.
Venus Ducatelli, p. 8, pl. 4, f. 2, Jan. 1838.
Venus inoceriformis Wagner, p. 70, pl. 40, f. 1 ; J. Ac. viii, p. 51, pl. 1, f. 1, 1839.
Venus latilirata, p. 68, pl. 38, f. 3 ; Proc. Ac. i, 28, July 1841.
Venus latisulcata, p. 3, cover of No. 1, Apr. 16, 1839 ; p. 40, pl. 20, f. 6 (type genus Euloxa Conr. 1864).
Venus Mortoni, p. 8, pl. 5, f. 1 ; J. Ac. vii, 251, 1837.
Venus permagna, p. 9, Jan. 1838
Venus Rileyi, p. 9, pl. 6, f. 1, Jan. 1838.
Venus tetrica, p. 7, pl. 4, f. 1, Jan. 1838.

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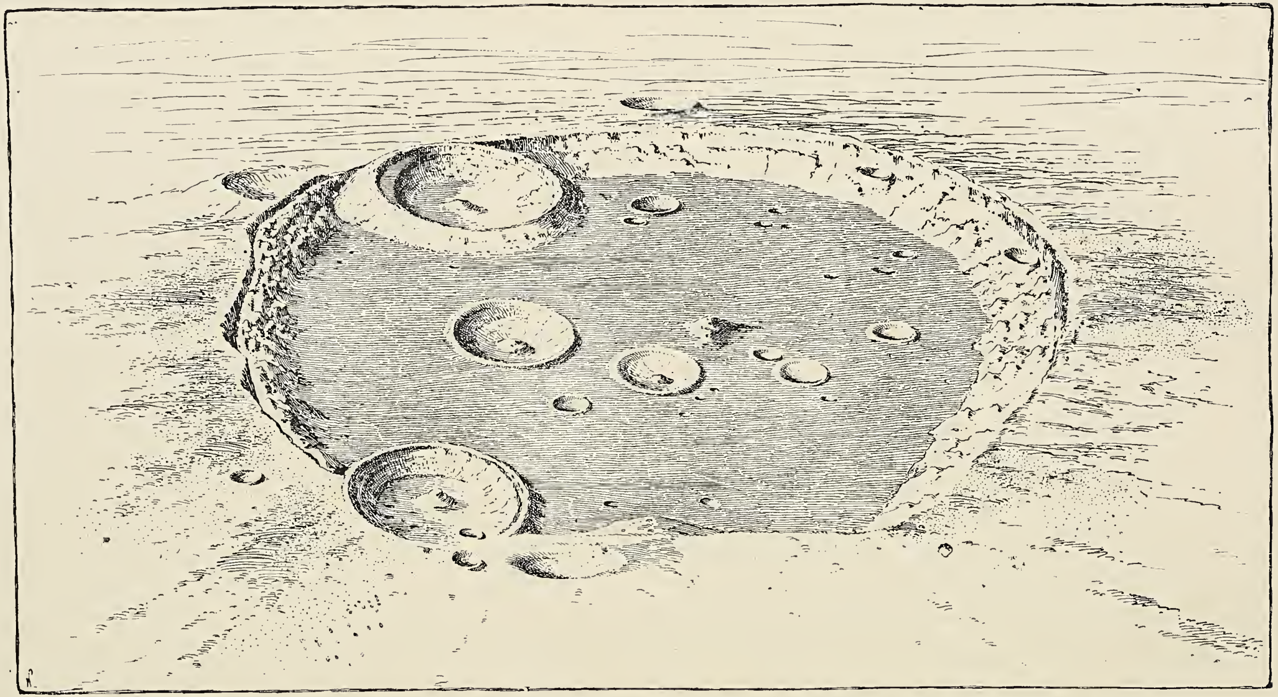

LUNAR CRATER CLAVIUS, SHOWING GROUPING OF CRATERS. DIAMETER 143 MILES; DEPTH ABOUT TWO MILES.

THE MOON'S FACE;

A STUDY OF THE ORIGIN OF ITS FEATURES.

BY

G. K. Gilbert.

ADDRESS AS RETIRING PRESIDENT.*

Delivered December 10, 1892.

The face which the moon turns ever toward us is a territory as large as North America, and, on the whole, it is perhaps better mapped. As its surveyor, even if armed with the most powerful of telescopes, is still practically several hundred miles away, his map does not represent the smallest features; but as all parts are equally accessible and as he has labored industriously these many years, there is no remaining space on which to write the legend "unexplored." Upon his map are a score of great plains with dark floors, which he calls *maria;* there are a score of mountain chains; there are a few trough-like valleys remarkable for their straightness; there are many thousand circular valleys with raised rims, which it is convenient this evening to call craters,† although for the purposes of detailed description

*A supplementary communication on the same subject was made to the Society at the meeting of January 7, 1893. The substance of that communication, as well as the results of later studies and experiments, are included in this publication.

An outline of the discussion was read to the National Academy of Science in November, 1892, and was reported in abstract in the American Naturalist, vol. 26 (1892), pp. 1056–1057. A similar outline was presented to the New York Academy of Science in February, 1893, and is reported in abstract in vol. 12, pp. 93–95, of the Transactions. The same abstract appeared in Astronomy and Astro-Physics for March, 1893, No. 113, p. 286.

† The word *crater*, derived from the Greek name of a kind of bowl, is used chiefly to designate the bowl-shaped cavities of volcanoes. In this paper, as in most selenographic writings, it designates a topographic form without implication as to the origin of the form.

he has found it convenient to give them many distinctive names;* there are thousands of bright streaks, which are neither ridges nor hollows, but mere bands of color; there are many hundred narrow linear depressions, which he calls rills.

Despite the persistent enthusiasm, the patience, and the industry with which he has studied his field, it must nevertheless be admitted that he has rarely satisfied himself and never satisfied his fellow-workers with the explanations he has suggested as to the origin of the features his map delineates. But selenographers are not the only students of the moon's face. There are also selenologists, who use the telescope comparatively little but cogitate much, and who have evolved theories of great ingenuity and variety. Far be it from me to say aught to their disparagement, for this evening I join myself to their ranks; but, again, it must be confessed that the selenographers do not look upon the teachings of the selenologists with favor. So, despite all that has been done, the field of theory is still open, and this is my excuse for putting forth ideas founded neither on protracted observation † nor on protracted study—this and the further plea that the problem is largely a problem of the interpretation of form, and is therefore not inappropriate to one who has given much thought to the origin of the forms of terrestrial topography.

Crater Characters.—In the study of lunar physiography—or physiognomy, if you prefer—interest naturally centers in the craters, for these are the dominant features. All theories begin with them; and, before examining the theories, it will

* Neison classified craters as crater cones, crater pits, craterlets, craters proper, crater plains, ring plains, mountain rings, and walled plains, recognizing gradation between them and also between walled plains and maria. The Moon and the Condition and Configuration of its Surface, by Edmund Neison, London, 1876.

† My observations were practically limited to two lunations in August, September, and October, 1892, a period affording eighteen nights available for work. My instrument was the 26½-inch refractor of the United States Naval Observatory, and the power found most serviceable was 400.

be well to place clearly in view the characteristics of the lunar craters. The range in size is great, extending from a maximum of about 800 miles diameter to a minimum of less than one mile. The size of the smallest ones is not known, as they are beyond the present power of the telescope. Within this range are several varieties, more or less correlated with size, but their intergradation is so perfect that they are all regarded as phases of a single type.* Those of medium size will be first described.

Picture to yourself a circular plain ten, twenty, fifty, or one hundred miles in diameter, surrounded by an acclivity which everywhere rises steeply but irregularly to a rude

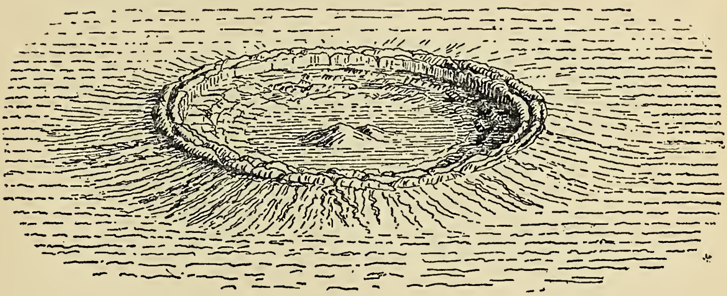

Fig. 1.—Type form of lunar crater.

terrace, above which is a circular cliff likewise facing inward toward the plain. This cliff is the inner face of a rugged, compound, annular ridge, composed of shorter ridges which overlap one another, but all trend concentrically. Seen from

* The only exceptions to the type that I have noted are associated with certain of the rills. They are so small that I could not determine their characters with certainty, but they seemed to lack rims and to be hopper-shaped.

Neison (op. cit., p. 66) describes "crater cones" as of different type, characterized by cups at the apices of cones, but these I did not succeed in discovering. On several occasions I saw at the terminator what appeared to be small craters perched on high pedestals, but when the same objects were observed at such distance from the terminator as to escape the exaggeration peculiar to that illumination, they were seen to be depressed craters of the usual type.

above, this ridge calls to mind a wreath, and it has been so named. From the outer edge of the wreath a gentle slope descends in all directions to the general surface of the moon, which it is convenient to call here the outer plain. The outer slope of the crater may be identical in surface character with the outer plain, or it may be radially and somewhat delicately ridged, as though by streams of lava. The inner slope, from the base of the cliff to the margin of the

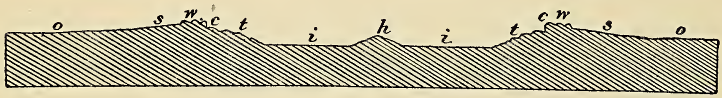

Fig. 2.—Cross-profile of lunar crater. *oo* = outer plain; *ss* = outer slope; *ww* = wreath; *cc* = inner cliff; *tt* = terraced inner slope; *ii* = inner plain; *h* = central hill.

inner plain, is broken by uneven and discontinuous terraces, which have the peculiar habit of land-slip terraces as one sees them about the flanks of a plateau capped by a heavy sheet of basalt. From the center of the inner plain rises a hill or mountain, sometimes symmetric but usually irregular and crowned by several peaks. From the outer plain to the base of the wreath the ascent is 1,000 or 2,000 feet, and the ascent thence to the top of the wreath may be as much

Fig. 3.—View and section of margin of basaltic table, showing landslip terraces.

more. The descent from the wreath to the inner plain is ordinarily from 5,000 to 10,000 feet, and the height of the central hill is 1,000 to 5,000 feet. With rare exceptions, the inner plain is several thousand feet lower than the outer plain.

The central hill is not universally present, but appears in rather more than half the craters of medium size. With craters more than 100 miles in diameter its occurrence is

comparatively rare, and it disappears altogether before the maximum size is reached. Increase in size is also accompanied by atrophy of the wreath, but to this rule there is a conspicuous exception in that the greatest of all the craters preserves the third part of its wreath. In the absence of the wreath there is no sharp line of demarcation between the craters and the maria, and several large plains of oval outline, originally named as maria, are now classed with the craters.

The inner plain is a constant feature with craters of maximum and medium size, but disappears as diameter diminishes. It is rare with diameters of less than five miles. The central hill also is obsolescent downward, but persists farther than the inner plain. To my eye the interiors of most craters under four miles and of all under two miles appear as simple cups.* The wreath of small craters is correspondingly small and is apparently a simple ridge, but it does not disappear. The depth of crater varies with the width, but less rapidly, so that the small have less absolute but greater relative depth.

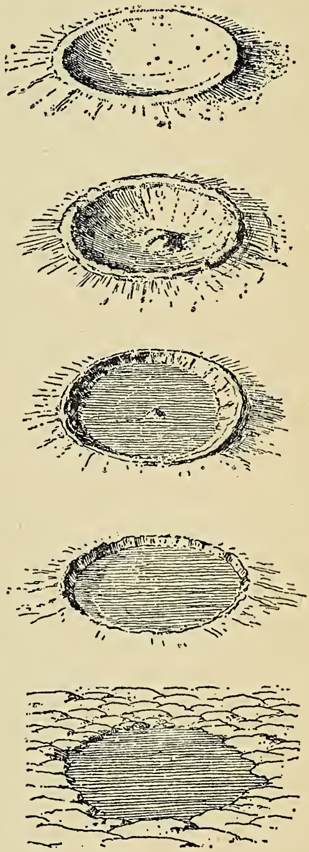

FIG. 4.—Varieties of lunar craters as related to size. The uppermost sketch represents the form of the smallest craters, the lowermost the form of the largest.

The craters are more abundant in some regions than in others, and there are comparatively few upon the maria.

*I am conscious that as the limit of telescopic vision is approached, the details of craters must disappear before the craters themselves are lost, and am therefore anxious to have this observation verified by those who are able to use higher powers than I could.

Usually the small craters are far more numerous than those of medium size, but in certain districts well covered by craters those of small size are less abundant. The craters overlap one another with every conceivable relation, except that the overlapping is never reciprocal. It is in every case possible to distinguish the newer from the older, the older being partially effaced by the newer. Small craters occur on all parts of larger ones, not excepting the wreath and the steep inner slope (see Plate 3).

Volcanic Theory.—By the majority of writers the craters are assumed to be volcanic, and as they differ in size, abundance, and form from terrestrial volcanoes, it is thought that they represent some special type of volcanism determined by physical conditions peculiar to the moon. Let us compare the lunar and terrestrial craters and see how far their differences can be explained as dependent on differences of physical condition.

Take first the difference in abundance. Faye estimates the number of visible lunar craters, of all dimensions, at 20,000 to 30,000. There is no equivalent area on the earth so well explored as to admit of a close estimate, but a general idea may be derived from our knowledge of North America. From personal observation of the principal volcanic districts in Utah, Nevada, Arizona, and New Mexico, I estimate the number of craters and ruins of craters in those States and Territories at 1,000. In the remainder of our western mountain region there are probably 500 more; 500 may safely be ascribed to the districts of similar geologic type in Alaska and British Columbia, and 1,000 to Mexico and Central America; giving as an estimate for the continent 3,000 craters, or one-tenth of Faye's larger estimate for a lunar area of similar extent. Our estimate includes only the craters formed at so recent a date that the processes of erosion and deposition have neither demolished nor buried them; but the geologic record shows that there have been volcanic eruptions in all ages, and indicates as probable that every district has been at one time or another a field of volcanic

activity. Had the terrestrial craters of all periods been exempt, like those of the moon, from atmospheric and aqueous attack, it is easy to imagine that they might now be equally abundant.

In comparing the sizes of craters on moon and earth, it is impossible to consider general averages, because the size of the moon's smallest is not known; there is so great a multitude close to the limit of telescopic vision that we can readily believe there is another multitude beyond. Only the maxima can be compared. The largest known terrestrial crater has a diameter of about 15 miles; the largest lunar crater, that whose rim is partially preserved in the Carpathian-Apennine-Caucasus chain of mountains, had a diameter of 800 miles. The ten largest terrestrial craters of which I have record have a mean diameter of 11 miles; * the mean for the ten largest lunar craters is 275 miles.† The ratio of the largest is as 53 to 1; the ratio for the ten is as 25 to 1. The

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* (1) The old crater containing Lake Bombon, Isle of Luzon, is mapped (Reclus) as 16 x 14 miles in extent. (2) The crater of Asosan, Isle of Kiushiu, Japan, is 15 miles across (Milne). (3) Scrope mentions a "circulàr crateriform lake, about 15 miles in diameter" in northern Kamchatka (Volcanoes, 2d ed., London, 1862, p. 457). (4) An imperfect crater cirque on Mauritius, mentioned by Charles Darwin, is mapped (Admiralty) as about 15 x 11 miles in extent. (5) The crater walls surrounding Lake Bolesna, Italy, are mapped as 11 x 9 miles across. (6) The extent of the crater containing Lake Maninju, Sumatra, is mapped (Reclus) as 15 x 7 miles. (7) The extent of the explosion crater of Pepandayan (Junghuhn, cited by Scrope) is 15 x 6 miles. (8) The ancient crater of Teneriffe has a smaller diameter, variously cited as 7 and 8 miles and variously mapped as 7 and 10 miles. The larger diameter exceeds the smaller by 40 per cent. (9) The crater of Deception Island (S. Shetland) is mapped as 8 x 7 miles in extent. (10) The basal rampart of Monte Cavo, Italy, is 7 miles across. Certain maps indicate a basal rampart, 9 miles across, about Mt. Marindin, Isle of Mindanao, and a similar rampart, 10 miles across, about Mt. Askia, Iceland, but confirmatory literature has not been discovered.

† Apennines, Serenitatis, Crisium, Humorum, Humboldtianum, Bailly, Iridum, Clavius, Otto Struve, and Grimaldi. I have some doubt as to the propriety of including Mare Humorum, and less doubt as to the omission of Mare Tranquilitatis and Mare Fecunditatis. The mean diameter of each crater was used in the computation.

import of these ratios is materially modified by considerations arising from the laws of gravitational attraction. Taking into account the relations of the moon's mass and radius to the earth's mass and radius, it is computed that downward attraction at the moon's surface is only one-sixth as great as at the earth's surface. Bodies of the same size and material weigh only one-sixth as much on the moon; a bomb projected with the same energy or initial velocity would fly six times as far, and a cliff of the same material may stand six times as tall; so a lunar crater, if produced in the same way, may be six times as broad or deep as a crater on the earth without exciting our wonder. Applying the factor 6 to the ratios just cited, we reduce them severally to 9:1 and 4:1.

Fig. 5.—Diagram showing the relative diameters of the ten largest terrestrial craters (*E*) and the ten largest lunar (*M*). The lunar diameters are divided by six. The diameters are represented by the distances from the vertical line at the left to the short vertical lines.

To these ratios, considered as obstacles to the acceptance of the volcanic theory of lunar craters, three comments are pertinent:

(1) The individual terrestrial diameters on which the second ratio is based are closely grouped near their maximum (Fig. 5), as though constrained by a limiting condition; the individual lunar diameters are widely scattered near their maximum, like the distances of aberrant shots from the bull's-eye. Reasoning from these facts of distribution, we should predict that the complete exploration of the earth will bring to light other craters about as large as those now known, but will discover none much larger; but we could not make a similar prediction as to the maximum crater on the opposite side of the moon.

(2) The conditions affecting volcanic action in the earlier

geologic periods were doubtless different from those determining the size of the craters we can examine.

(3) The material of the moon may differ from that of the earth's crust in such way as to affect the size of volcanic craters.

In vertical dimensions there is no important discrepancy. Lunar craters of the first rank range from 8,000 to 15,000 feet in depth; terrestrial, probably from 2,000 to 4,000. Dividing the lunar measures, as before, by six, we obtain 2 : 3 as the ratio of lunar depth to terrestrial; but as few terrestrial craters have been measured, this result cannot claim high precision.*

The contrasts as to form are of greater importance. To set them forth fully it is necessary to give separate consideration to several types of terrestrial craters. These may be called the ordinary or Vesuvian, the Hawaiian, and the maar types. Craters of the Vesuvian type—and these include nineteen-twentieths of all terrestrial volcanoes—are formed of lavas containing a considerable amount of water, and usually result from extravasation and explosion in alternation. As the lava rises in its conduit the contained water is converted into steam, by which the lava is torn to fragments and thrown into the air. That which falls back into the vent is again thrown upward, and that which falls outside the vent builds the crater rim. From time to time drier lava wells up and overflows the rim, or else forces a way to the surface at some lower level. In this manner there is accumulated a conical mountain with a funnel-shaped cavity at the top. Eruption is not continuous, but is interrupted by periods of quiescence, and sometimes, after a long interval of quiet, operations are again initiated by a great explosion of steam, the upper portion of the cone being blown out and an immense cavity left in its place.

* The greatest measured depth of a terrestrial crater with which I am acquainted is that of the cup holding Crater lake, Oregon—3,000 feet. Pichincha has been estimated at 3,000 to 4,000 feet. The general depth of the lunar crater Theophilus is 15,300 feet (Ebert).

Eventually the reissue of lava builds a new cone inside the great crater, and this cone, which always carries a crater at top, may grow so as to bury completely the wreck of the great explosion.

With the forms resulting from this process, or alternation of processes, the lunar craters have little in common. Ninety-nine times in one hundred the bottom of the lunar crater lies lower than the outer plain; ninety-nine times in a hundred the bottom of the Vesuvian crater lies higher than the outer plain. Ordinarily the inner height of the lunar crater rim is more than double its outer height; ordinarily the outer height of the Vesuvian crater rim is more than double its inner height. The lunar crater is sunk in the lunar plain; the Vesuvian is perched on a mountain top. The rim of the Vesuvian crater is not developed, like the lunar, into a complex wreath, but slopes outward and inward from a simple crest-line. If the Vesuvian crater has a central hill, that hill bears a crater at summit and is a miniature reproduction of the outer cone; the central hill of the lunar crater is entire, and is distinct in topographic character from the circling rim. The inner cone of a Vesuvian volcano may rise far higher than the outer; the central hill of the lunar crater never rises to the height of the rim and rarely to the level of the outer plain. The smooth inner plain characteristic of so many lunar craters is either rare or unknown in craters of Vesuvian type. Thus, through the expression of every feature the lunar crater emphatically denies kinship with the ordinary volcanoes of the earth. If it was once nourished by a vital fluid, that fluid was not the steam-gorged lava of Vesuvius and Etna.

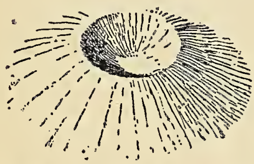

Fig. 6.—Crater of Vesuvian type, without central cone. Features due to erosion are omitted.

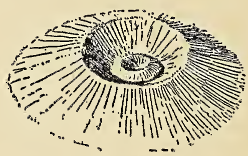

Fig. 7.—Crater of Vesuvian type, with central cone. Features due to erosion are omitted.

Craters of the Hawaiian type are produced by lavas containing so little moisture that its conversion into steam does not cause violent explosions. Successive eruptions, flowing from the orifice in different directions, build by their congelation a massive cone with crater at top. In the intervals between eruptions the lava stands in the crater as a pool or lake, the liquidity of which is maintained by a circulation, through the conduit, of super-heated lava from below. If the circulation slackens, a crust forms over the lake, giving the crater an inner plain like those of the moon. If the current is more active, the molten lava remelts part of the lava of the cone, undermining the walls of the crater and causing them to fall in, whereby the cavity and the lake are enlarged. Partially fallen fragments of the crater wall constitute terraces of the land-slip type. Sometimes the lava retreats downward after crusting over, and fragments of the crust, adhering to the crater walls, form terraces of another type. Craters of this sort are somewhat rare, but their rarity does not affect their value as interpreters of extra-telluric phenomena. As long ago pointed out by Dana, they resemble the moon's craters much more closely than do those of ordinary volcanoes. They agree with lunar craters in the possession of inner plains, and to a certain extent in the terracing of their inner walls. They differ in the fact that they occupy the tops of mountains; in the absence of the wreath; in the absence of the central hill, and usually in the presence of level terraces due to the formation of successive crusts. In my judgment the differences far outweigh the resemblances, and I have not succeeded in imagining such peculiarities of local condition as might account for the divergence in form.

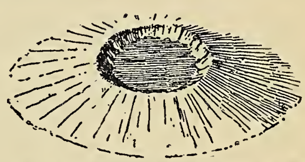

FIG. 8.—Crater of Hawaiian type. Features due to erosion are omitted.

The maars are of still rarer occurrence, and represent the antithetic phase of volcanism. The process of their formation includes no eruption of lava, but merely an explosion

of steam. By that explosion a body of rock is broken into fragments and thrown outward. Such of the fragments as descend outside the cavity are heaped about its margin, constituting a rim, which is smooth if the fragments are small and rugged, and irregular if they are large. Less than fifty craters of this type are known, and they are all small, the largest being less than two miles wide. They resemble the craters of the moon, in that their bottoms are depressed below the general level, and in that the volumes of their rims are approximately equal to the capacities of their cavities. They lack the wreath, the inner terraces, the inner plain, and the central hill. Thus characterized, they differ widely from the lunar craters of medium and maximum size, but they resemble those of smaller size. It is possible that the resemblance depends in part on the invisibility of minor features of the small craters of the moon; but it is perhaps equally possible that better seeing would disclose yet other elements of similarity.

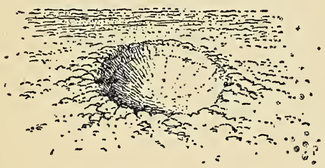

Fig. 9.—Crater of maar type.

If we accept this resemblance as satisfactory, half of the moon's hollows are explained. Can we adjust the explanation to the remaining half, or must we draw an arbitrary line through what appears to be a continuous series of phenomena and study the two parts separately? To adapt the explosive hypothesis to the larger craters it is necessary not merely to think of a greater explosion, but to imagine some phase or accompaniment of explosive action which will furnish the rim with a system of concentric ridges and the cavity with a level bottom and a central eminence. If the attempt at adaptation fails, as I think it must, then the explanation can be accepted for the small craters only by divorcing them from the large—and, whether it be accepted or not, inquiry must be continued.

Before passing to the examination of other theories, it is

well to bring together the results of our inquiry into the adequacy of the volcanic. The comparative abundance of lunar craters is readily accounted for without prejudice to the theory. Their greater maximum width, though partly referable to a gravitational factor, constitutes a real difficulty, especially as volcanoes appear to have a definite size limit, while lunar craters do not. Form differences effectually bar from consideration all volcanic action involving the extensive eruption of lavas, whether dry or saturated with water. They also exclude the maar process (single explosion) as an explanation of medium and large craters, but not as an explanation of small craters. The volcanic theory, as a whole, is therefore rejected, but a limited use may be found for the maar phase of volcanic action in case no other theory proves broad enough for all the phenomena.

Tidal Theory.—Of other theories, a few are somewhat related to the volcanic. It is hardly profitable to discuss the suggestion that the greater walls were formed about the vortices of a primeval liquid moon,* nor the suggestion, albeit advanced independently by several authors, that the vast circling cliffs of the moon are remnants of Cyclopean bubbles that have burst.† But an ingenious tidal theory, which appears to have sprung up independently in France, Germany, and England,‡ merits careful examination. It postulates a time when the moon was liquid, with the exception of a thin crust. The moon then rotated more rapidly than now, and great tides, excited by the earth's attraction, racked and cracked its crust and here and there squeezed out a portion of the liquid nucleus, which flowed back again when the tidal wave had passed; but congelation caught

* Rozet: Mémoire sur la Sélénologie. Comptes rendus, vol. 22 (1846), p. 470.

† Robert Hooke: Micrographia (1667) [not seen]. Jules Bergeron: L'Astronomie (1882), p. 346; (1883), p. 112. A. St. Clair Humphreys: Jour. Brit. Astr. Ass., Dec., 1891, p. 132.

‡ Faye: Rev. Scientifique, 27 (1881), p. 130. H. Ebert: Ann. Physik und Chemie, vol. 41 (1890), p. 351. J. B. Hannay: Nature, vol. 47 (1892), p. 7.

the flood at its edges, so as to mark its limit by a solid ridge. By each successive tide the operation was repeated, with the result that the wall was given a circular form and was gradually built up. The process was finally closed by the congelation of lava in the orifice, and while congelation was in progress the last feeble eruption sometimes produced a central hill.

In certain respects this theory is well founded. It is true that the earth is able to produce far greater tides on the moon than the moon produces on the earth, and if we may accept the conclusion of G. H. Darwin that the moon is retreating from the earth,* then the reciprocal tides of moon and earth were greater at an earlier date than they are now. That a circular ridge may be built up by the alternate extrusion and retraction of a suitable substance through an orifice has been demonstrated by Ebert, who devised apparatus and conducted a series of experiments. The crater rims he achieved sloped regularly outward and were steep and rudely terraced inward, thus reproducing the more important features of the lunar rims, with the exception of the wreath, and by special manipulation he was able to approach the characters of the wreath.

In other respects the theory finds less support. At the time of formation of the larger craters the crust must have been thick and strong to sustain the weight of their rims. It could not then or afterward have been divided by a close plexus of cracks, but such a plexus seems necessary under the theory to account for the multitude of small craters which overlie the large. Again, it is pertinent to inquire whether the crustal strains engendered by great tides in a liquid nucleus would find relief in the postulated manner. If the crust were divided by fissures, would not the tensile strains wrought by the crest of the tidal wave cause the fissures to gape, instead of forcing out the liquid through apertures here and there? Or, if there were no fissures, would not the strains suffice to produce them? The postu-

*Article "Tides" in Encycl. Britannica, vol. 23, p. 378.

lated normal stresses are measured by lava columns from 5,000 to 15,000 feet in height, and the tangential strains resulting from the greater of these stresses would rend a crust of granite 100 miles thick.* Yet, again, there are numerous craters of small or medium size occupying slopes of the greater crater rims, and the initiation of these by tidal process seems impossible. Whatever lava escaped from an orifice on a slope would flow down the slope instead of being drawn back.

Snow Theory.—Another theory assumes that the moon is covered with snow or ice. The site of each crater was once occupied by a pool of water which by heat from below was vaporized. The vapor was quickly converted into snow, part of which fell back in the pool to be vaporized again, and all of which was eventually accumulated in an annular ridge.†

There is some reason to question the existence of water and ice on the moon's surface, but as this subject will presently be considered in another connection, the point will be waived here and attention restricted to questions of form and relation. If the rim were built up by the quiet fall of an infinitude of ice particles or snow flakes, its configuration should be smooth and regular instead of exhibiting the rugosity actually observed. The postulated heat of the central area might render the inner slope steep and even produce the inner cliff and terraces, but the theory affords no explanation of the wreath nor of the central hill. It fails likewise to account for the small craters formed on the rims and slopes of the larger, for the bottoms of these are far above the assumed rock plain of the moon through which

* The computation on which this statement is based assumes 2.75 as the density of the lunar lava and 11,000 pounds per square inch as the tensile strength of granite. It assumes also that all parts of a vertical section of crust are subjected to the same strain, but any tidal deformation of the crust would make the distribution of strain unequal.

† John Ericsson: Nature, vol. 34 (1886), p. 248. S. E. Peal: Nature, vol. 35 (1886), p. 100; English Mechanic and World of Science, vol. 47 (1888), p. 477.

the theory supposes the internal heat to have been communicated.*

Meteoric Theories.—All other theories which I have been able to discover appeal in one way or another to the collision of other bodies with the moon's surface, and for want of a better term I shall call them meteoric. If a pebble be dropped into a pool of pasty mud, if a raindrop fall upon the slimy surface of a sea marsh when the tide is low, or if any projectile be made to strike any plastic body with suitable velocity, the scar produced by the impact has the form of a crater. This crater has a raised rim, suggestive of the wreath of the lunar craters. With proper adjustment of material, size of projectile, and velocity of impact, such a crater scar may be made to have a central hill. Thus scars of impact may simulate in many ways the scars of the moon's face, and a number of theories have accordingly been broached which agree in regarding the craters as due to the bombardment of the moon by projectiles coming from without. As the present study is primarily physiographic, these similitudes of form have been considered with great care, and it is my belief that all features of the typical lunar crater and of its varieties may be explained as the result of impact. The special considerations presently to be adduced are along this line.

Long ago it was suggested that the projectiles might have been fired from terrestrial volcanoes, but the speed actually acquired by the ejecta of volcanoes falls so far short of that necessary to carry them beyond the sphere of the earth's attraction that this view is no longer entertained. All other suggestions have regarded the material as cosmic. Every shooting star records by its brief coruscation the collision with our atmosphere of a particle of star dust; and though the number of these which can be seen by a single observer

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* These statements may do injustice to Peal's version of the theory, which has been given to the world only in abstract. I have not seen his fuller exposition in a pamphlet privately printed in India with an edition of 100.

in one night is not great, it has been computed that no less than 400 millions are captured by the earth in the course of twenty-four hours. So minute are they in general that their ashes do not contribute to the earth's surface an appreciable layer of dust; but a few have such size that they are not completely consumed in traversing the atmosphere and fall to the earth as aërolites weighing grains, ounces, pounds, or even tons. For the most part they strike the atmosphere with a velocity far higher than could be induced by the earth's attraction, and we must believe that they are speeding through space in all directions in numbers that defy the imagination. They must collide with all planetary bodies in numbers depending chiefly on the area of surface exposed, and the moon, of course, receives its share.*

As the moon either is without atmosphere or has one of extreme tenuity, the mechanical effect of this bombardment may be important, for the average velocity of the meteors is from fifty to one hundred times as great as that with which the swiftest ball leaves the cannon, and the energy of a projectile is measured by the square of its velocity. Nevertheless it is incredible that even the largest meteors of which we have direct knowledge should produce scars comparable in magnitude with even the smallest of the visible lunar craters. Recognizing this difficulty, advocates of meteoric theories have assumed that at some earlier period the meteors encountered by our solar system were of greater size than now, and as no evidence has been found that the earth was subjected to a similar attack, there is assigned to the lunar bombardment an epoch more remote than all the periods of geologic history, any similar scars produced on

* I have discovered no published statement of meteoric theories more than twenty years old, but the idea is older and various obscure allusions indicate that it was earlier in print. Proctor makes a meteoric suggestion in 1873 (The Moon, p. 346), and advocates it in 1878 (Belgravia, vol. 36, p. 153). A meteoric theory is said to be contained in *Die Physiognomie des Mondes*, by "Asterios," Nordlingen, 1879. A. Meydenbauer advances another in Sirius, for February, 1882, and he includes bodies other than cosmic.

the earth having been obliterated by the processes which continually reconstruct and remodel its surface.

Another difficulty has been found in imagining a condition of lunar surface which should admit at the same time of plastic molding and of the preservation of the resulting forms. The steep inner slopes of lunar craters are, in places, from 15,000 to 20,000 feet in height; their stability in the presence of even the feeble gravitational force at the moon's surface demonstrates great strength of material, and the mind does not readily associate great strength with plasticity. To avoid this difficulty it has been assumed by more than one student that the moon's surface was soft when the craters were made; but it seems to me that this assumption does not really escape the difficulty, for it will not do to postulate a degree of softness incompatible with the survival of lofty cliffs. To my mind it appears that the difficulty is only imaginary and not real. Rigidity and plasticity are not absolute terms but relative, and all solids are in fact both rigid and plastic. The apparent contrast between the two properties belongs to the laboratory and to those phenomena of nature involving small masses and small forces. When great masses and great forces are involved, as, for example, in the making of continents and mountain chains, the distinction loses value. The phenomena of mountain structure demonstrate that under sufficient strains great bodies of rock both bend and flow. If the lunar craters were produced by collision, the masses of matter involved were greater than those of terrestrial mountain ranges and the concentration of energy was correspondingly great. Moreover, a portion of this energy may have been converted into heat, with the result that the parts affected were rendered less rigid or even molten, and it even appears necessary to assume a result of this sort in order to account for the level surfaces of the inner plain of the craters. My friend, Mr. R. S. Woodward, has kindly made for me some computations which serve to illustrate this point. If a body fall to the moon's surface from an infinite distance, being influenced only by the

attractive power of the moon, its velocity on reaching the surface will be one and one-half miles per second; and the equivalent energy, if all converted into heat and all stored in the mass of the falling body, would suffice to raise its temperature, supposing it to consist of ordinary volcanic rock, through 3,500 degrees of the Fahrenheit scale. In other words, the quantity of heat developed would be greater by one-half than that necessary to fuse the body. The average velocity of shooting stars is estimated at 45 miles per second, or thirty times that of a body falling freely to the moon, and it is easy to understand that the heat developed by the sudden arrest of a fragment of rock traveling with such speed might serve not only to melt the fragment itself, but also to liquefy a considerable tract of the rock mass by which its motion was arrested.

It is convenient to mention in this place a special phase of the meteoric theory which, though not devised to avoid this difficulty, nevertheless does avoid it. Meydenbauer, as a corollary of certain conclusions in regard to meteoric matter, holds that the surface of the moon is clothed with a mantle of cosmic dust, a deep layer of loose particles everywhere concealing the solid nucleus, and that the fall thereon of aggregates of similar dust produced the lunar craters. By experimention with various finely divided substances he has in this way produced small craters simulating several of the lunar varieties. His results show raised rims analogous to the lunar wreath, central hills, and arched inner plains, such as characterize a few of the lunar craters. His published results do not include level inner plains, nor the association of inner plains with central hills; but, on the other hand, he does not extend this process to the largest craters and the maria. For them he suggests the collision of solid stars of sulphur or phosphorus, originally moons of the earth's system, and he recognizes fusion as one of the results of their collision.*

The third difficulty is found in the relation of the volume

* A. Meydenbauer: Sirius, February, 1882.

of the rim to the capacity of the hole. If the collision produced no condensation of the lunar tract affected (and condensation would be anticipated only on the hypothesis of cosmic dust), we should naturally expect to find in the rim the entire volume of matter displaced in the formation of the hollow plus the volume of the moonlet; but this relation does not appear to obtain. The impression derived from telescopic observation and the inspection of photographs is that the rims of some craters are commensurate with the hollows, while the rims of others are not, and this impression is confirmed by computations based on the measurement of shadows. Ebert has compiled the available published data and computed the ratio of rim content to cavity content for ninety-two craters, ranging in diameter from 8 to nearly 100 miles.* In twenty-eight instances he finds the rim content the greater; in the remaining sixty-four instances he finds it the smaller; and in about fifteen instances the rim volume is but a small fraction of the content of the cavity. He finds further that the rim is relatively small or the cavity relatively large in the case of the larger craters. Though the imperfection of the data gives a large probable error to the determinations, there can be no question of the general fact that in many instances the rims of large craters are quite inadequate to fill the cavities they surround. This is an important fact, but it is not necessarily inimical to the impact theory. In the course of a series of laboratory experiments, in which craters were produced by throwing projectiles of various plastic materials against targets of similar materials, it was occasionally found that the rim when pared away would not fill the hollow, and the cause of this result was discovered. When target and projectile were of uniform consistency throughout, there was no defect of rim; but when the general mass of the target was softer than the portion at the surface, the uplift consequent on the production of the hollow was only partly localized about its

* H. Ebert: Ueber die Ringgebirge des Mondes. Sitzungsberichten d. Physik.-med. Societät. Erlangen, p. 171. Munich, 1890.

periphery, the remaining part being widely distributed through flow of the softer material below. It is possible, therefore, to interpret the quantitative relations discovered on the moon in terms of local physical condition without rejecting the impact theory.

A fourth difficulty is connected with the circular contours of the craters. If a ball of mud be allowed to fall vertically upon a horizontal surface of the same material, the resulting crater is circular; but if instead it be thrown obliquely, the resulting crater has an oval contour. Except for irregularities which may be counted as details of form, some of the lunar craters are as nearly circular as can be determined by measurement; others are slightly elliptic; a few only are notably elongate. It is inferred that the predominant direction of the incident bodies supposed to have formed them was vertical to the lunar surface or nearly so; but it can be shown from simple geometric considerations that the predominant angle of incidence of swift-moving meteoric bodies approaching from all directions would be 45 degrees, and the scars produced by such collisions would be predominantly oval instead of predominantly circular. So far as my reading has extended I have discovered but one suggestion for the obviation of this difficulty, and that was applied only to very small lunar craters. It was suggested by Proctor that immediately after the shock of collision there might be an elastic return to a circular form.* The idea requires for its realization a high tensile elasticity, such as we do not know in any rocks, but only in certain substances of organic origin, and it thus fails to receive support from the phenomena of our terrestrial experience. There has occurred to me an entirely different mode of escaping the difficulty, and as this is my personal contribution to the subject it will be set forth somewhat fully.

Moonlet Theory.—Besides the nomadic and apparently individual meteors of space, there are certain groups symmetrically arranged and moving in a systematic and orderly

* The Moon: London, 1873, p. 346.

way. One of these groups is arranged in the form of a ring and encircles the planet Saturn. This ring is broad and thin, and all parts of it lie nearly in one plane. The meteors which constitute it are so numerous that portions of the ring appear continuous and solid. They are too small to be individually perceived, but there can be little question that they all travel about the planet in a system of parallel orbits and with correspondingly adjusted velocities. It is my hypothesis that before our moon came into existence the earth was surrounded by a ring similar to the Saturnian ring; that the small bodies constituting this ring afterward gradually coalesced, gathering first around a large number of nuclei, and finally all uniting in a single sphere, the moon. Under this hypothesis the lunar craters are the scars produced by the collision of those minor aggregations, or moonlets, which last surrendered their individuality.

This change of conception yields a material difference in the law of the directions in which minor bodies approach the moon, the difference depending on the fact that all the minor bodies colliding with the greater body have initial orbits lying approximately in the same plane. To render this clear it is necessary to amplify the statement already made with reference to the predominant angle at which cosmic meteors encounter the surface of the moon. Their velocities are so high, as compared with the acceleration due to lunar attraction, that their courses in the vicinity of the moon may, without sensible error, be regarded as straight. The angle at which each one strikes the moon's surface depends upon the nearest distance of its produced orbit from the moon's center, and is entirely independent of the direction from which it approaches. We may therefore simplify the discussion of incidence angles by assuming that the meteors all come from the same direction and move along parallel lines. The number of meteorites being indefinitely large and their distribution entirely independent of the moon, we may for this purpose conceive them as an evenly distributed rain, of which the moon receives a certain por-

tion. This simplified conception is embodied in the diagram, Fig. 10. The angle of incidence is the angle included between the direction of the incident meteor and a line normal to the moon's surface at the same point. It is 0° at the center of the hemisphere turned toward the rain, and is 90° at the margin of that hemisphere. At any intermediate point, A, it is measured by the arc connecting that point with the center of the hemispherical surface. Through the point A draw a small circle in the plane parallel to the base of the hemisphere. It is evident that the zone of spherical surface above this plane includes the downfall of all meteors whose incidence angle is less than that of the meteors reaching A, and that the zone below it includes the downfall of meteors making greater angles. The number of those falling on the upper zone is measured by the area of the small circle. The number of those falling on the whole hemisphere is measured by the base of the hemisphere. The ratio of the one to the other, or the proportionate number of meteors having an incidence angle less than any given angle, i is equal to $sin^2\, i$. Substituting 30° and 60° successively for i, we learn that 25 per cent. of all the meteors have incidence angles less than 30°, and 75 per cent. have incidence angles less than 60°; so that 50 per cent. of the angles fall within the middle third of the quadrant. The law of distribution is graphically shown by curve A of Fig. 12, where abscissas represent angles of incidence, and ordinates the corresponding proportionate numbers of meteors. It will be noted that the number of meteors having incidence angles of 0° or 90° is a vanishing quantity, and that the incidence angle shared by the greatest number of meteors is 45°.

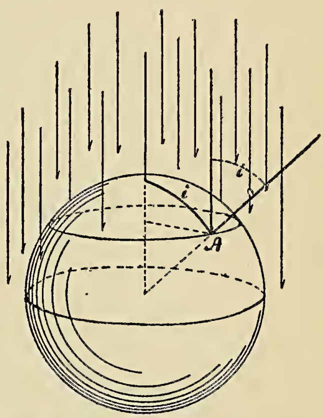

Fig. 10.—Diagram illustrating incidence angle of meteors.

In the case of moonlets reaching the moon from the plane of the postulated flat ring, all points of incidence would lie approximately in one plane, which plane would intersect the center of the moon. Postulating, as before, that the distribution of moonlets in this plane is equable, and that they move in parallel courses, and ignoring the attraction of the moon, we have the geometric relations shown in Fig. 11, and obtain *sin i* as an expression of the proportionate number of moonlets whose incidence angle is less than *i*. This differs from the expression obtained in the case of cosmic meteors, in that it involves the first power of the sine of the angle instead of the second, and there results a very different law of distribution, which is expressed by curve B of Fig. 12. In this distribution law the number of bodies incident at 90° is a vanishing quantity, but the number incident at 0° instead of being a vanishing quantity is a maximum, and one-half of all the moonlets have incidence angles less than 30°.

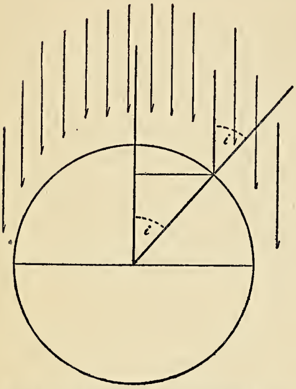

Fig. 11.—Diagram illustrating incidence angle of moonlets.

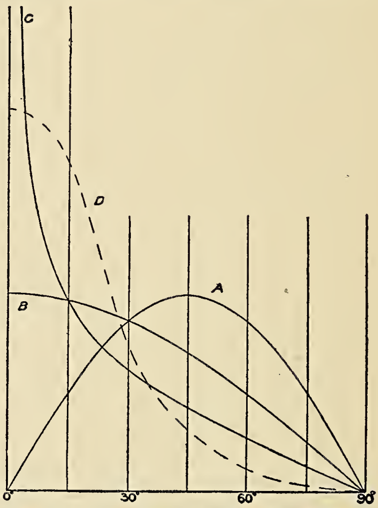

Fig. 12.—Distribution curves. Abscissas = angular deviation from verticality of bodies colliding with the moon. Ordinates = relative numbers of colliding bodies. *A* = curve for meteors. *B* = curve for bodies in a single plane. *C* = curve for moonlets, account being taken of the moon's attraction but not of the earth's. *D* = type of curve deduced from ellipticities of craters.

The law of incidence angle for ring-derived moonlets agrees with the law suggested by the roundness of the impact scars in that it

indicates a predominant approximation to verticality, and it therefore accords better with the phenomena than does the law of incidence angle derived from the theory of cosmic meteors. The introduction of the hypothesis of a Saturnian ring thus accomplishes much toward the reconciliation of the impact theory with the circular outline of the lunar craters. Whether it secures complete adjustment is not immediately apparent, and as the question of concordance or discordance is important to the impact theory, the discussion has been carried somewhat further.

The inquiry has followed three lines: First, an investigation of the ellipticity of lunar craters; second, an experimental investigation of the relation between incidence angle and ellipticity of impact craters; third, a more refined investigation of the orbital relations affecting the incidence angles of moonlets.

In the investigation of the ellipticity of the lunar craters I made use of a series of photographic negatives made at the Lick Observatory and deposited by the director of that observatory with the Smithsonian Institution. On these negatives the moon's disk has a diameter of from 5 to 5½ inches, so that measurements of some refinement can be made. It was found practicable to determine the conjugate diameters and through them the ellipticities of 120 craters. In three-fourths of these the ellipticity is less than .1; in eleven-twelfths it is less than .2; in twenty-nine-thirtieths it is less than .3.*

*Measurement was limited to the larger craters because the photographs did not indicate the outlines of the smaller with sufficient precision. Craters near the limb were ignored because inequalities in the heights of their rims might vitiate the results, and also because the perspective foreshortening makes it practically impossible to determine the directions of the longest and shortest diameters. Probably some of the measures made are affected by the latter difficulty and the ellipticities consequently underestimated, but the uncertainty thus introduced is thought to be less than would be added by throwing out all the craters obliquely seen and thus reducing the number of instances on which generalization is based. The number of measurements could be largely extended by direct telescopic observation.

In order to determine the angles of incidence corresponding to these ellipticities it was necessary to ascertain the general law subsisting between angle of impact and resulting ellipticity, and to this end a series of laboratory experiments was instituted. An apparatus was arranged by means of which a ball of plastic clay was made to strike a flat target of the same material at a measured angle and with determined velocity. The angle of incidence was systematically varied, the velocity of impact was varied, and the softness of the clay was varied. A crater similar in appearance to the smaller craters of the moon was readily produced, and it was found that ellipticity is a function not only of angle of incidence, but also of softness of material, and, inversely, of velocity of impact. No attempt was made to discover the precise character of this complex relation, because it was immediately evident that experiment could not be made to deal with velocities and strengths of material comparable with those associated with the production of the lunar craters. It was found, however, that ellipticity increases slowly with increase of incidence angle up to 30° or 40°, and with comparative rapidity for higher angles; and the comparison of this relation with observed lunar ellipticities led to the conclusion that from 65 to 90 per cent. of the lunar craters indicate incidence angles of less than 30°. As the theoretic distribution previously derived, on certain assumptions, assigns to but 50 per cent. of the moonlets angles within that limit, it appeared desirable to look more closely into the nature of the orbits of the moonlets as they approach the moon. This examination required mechanical conceptions and mathematic skill I was unable to supply, but I was so fortunate as to enlist the interest of our fellow-member, Mr. R. S. Woodward, who made an analytic investigation, on which the following paragraphs are based.

It will be recalled that parallelism of direction was assumed as a means of simplifying the discussion of incidence angle in the case of meteors and moonlets. In the case of meteors the assumption was fairly warranted, but not in

the case of moonlets. As the moonlets, by postulate, moved initially in orbits not very dissimilar from that of the moon and in the same direction, their initial velocities with reference to the moon were small as compared with the velocities created by the moon's attraction. Their courses in the near vicinity of the moon were therefore essentially parts of curved orbits of the nature of conic sections, with the moon at the focus, and could not properly be treated as equably distributed straight lines. Furthermore, the initial velocities of moonlets with reference to the moon, that is, the velocities with which they overtook or were overtaken by the moon, were not all the same, but were varied in a systematic way, being greater in proportion as the orbits of the moonlets differed from the moon's orbit.* Account was taken of these relations, but the influence of the earth's attraction, essential to a rigorous discussion, was ignored; the orbital equation of the moonlets was derived, the conditions of collision with the moon were examined, and the general expression for the angle of incidence was obtained.† This general expression is—

$$n = \sqrt{\sin i},$$

* Assuming circular orbits for moon and moonlet, calling their distances from the earth D and d, and calling their velocities in their orbits V and $V + u$, we have, from Kepler's third law,

$$\left\{\frac{(V+u)\,D\,\pi}{V\,d\,\pi}\right\}^2 = \left(\frac{D}{d}\right)^3, \text{ whence } \frac{D-d}{d} = u\,\frac{2\,V+u}{V^2}.$$

Since, in the case of any colliding moonlet, u is very small in comparison with $2\,V$, the fraction in the second member is sensibly constant, and u, the relative velocity of moonlet and moon, varies as $\frac{D-d}{d}$, or approximately as $D - d$, the distance between the orbits.

†At a distance c from the moon's center a moonlet has the initial velocity u. Placing the origin at the moon's center and the axis of reference parallel to the direction of the initial velocity, the polar equation to the moonlet's orbit is—

$$\rho = \frac{b^2\,u^2/\mu}{1 + \frac{a}{c}\cos\theta - \left(\frac{b}{c} - \frac{bu^2}{\mu}\right)\sin\theta},$$

where ρ is the radius vector, θ its inclination to the axis of reference, b the length of a perpendicular drawn to the axis from the point where the

n being the relative number of colliding moonlets whose angle of incidence is less than i. It indicates that 58 per cent. deviate less than 20° from the vertical, 70 per cent. less than 30°, and 80 per cent. less than 40°; and it yields the distribution curve marked C in Fig. 12.*

The theoretic distribution obtained by this partial treatment accords so well with the phenomena under discussion

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velocity is u, a the distance from the foot of that perpendicular to the origin, and μ the constant of gravitation for the moon.

The radius of the moon being r, the condition that the moonlet collides with the moon is—

$$u^2 < \frac{2\mu\left(r - \frac{r^2}{c}\right)}{b^2 - r^2}.$$

The general expression for the angle of incidence, i, is—

$$\sin i = \frac{b}{r\sqrt{1 + \frac{2\mu}{ru^2}\left(1 - \frac{r}{c}\right)}},$$

in which $2\mu/r = V^2 =$ the square of the velocity acquired by a body falling to the moon from an infinite distance. Since r/c is a small fraction and $2\mu/ru^2$ is a large number—

$$\sin i = \frac{bu}{rV} \text{ (nearly).}$$

By postulate (preceding note) u varies as b, and since r and V are both constant—

$$b = \sqrt{\sin i} \times \text{constant.}$$

*The curves A, B, and C of Fig. 12 represent the distribution of incident bodies with reference to angles of incidence under the laws expressed severally in the formulas:

$$n = \sin^2 i, \qquad n = \sin i, \qquad \text{and} \qquad n = \sqrt{\sin i},$$

n being the percentage of bodies whose incidence angle is less than i. The graphic representation of n in each case is the area beneath the curve from the vertical axis to the ordinate corresponding to i. The curves themselves represent the differential equations:

$$\frac{dn}{di} = 2 \sin i \cos i, \qquad \frac{dn}{di} = \cos i, \qquad \text{and} \qquad \frac{dn}{di} = \frac{\cos i}{2\sqrt{\sin i}},$$

in which dn is the relative number of bodies having the incidence angle i.

that greater refinement seems not to be required; but the theory of incidence angle nevertheless offers an inviting field to the mechanist. From the position reached in connection with the present study it seems probable that the moonlets originally moving in orbits outside the track of the moon would (mostly) reach the inner face of the moon (toward the earth), and the moonlets originally moving inside the moon's orbit would reach its outer face. Approaching the moon in this way, or in any other systematic way, the moonlets would determine and regulate the rotation of the moon. The motion of each moonlet at the instant of collision may be conceived as resolved into two components, one normal to the moon's surface and the other tangential. If the tangential component coincided in direction and velocity with the rotational motion of the moon's surface, the collision would not affect the moon's rotation; but if the tangential component had a velocity greater or less than the rotational motion, the moon's rotation would be accelerated or retarded. The aggregate result of all collisions would be such a rotation of the moon that its surface speed would equal the average of the tangential components of the velocities of moonlet impact. It is evident that if the tangential component of a moonlet's motion coincided exactly with the motion of the moon's surface, the impact phenomena would be the same as though the moonlet fell vertically on a motionless surface; and the harmonious adjustment of moon rotation to the motions of a system of moonlets would reduce to a minimum the ellipticity of craters.

In fine, the hypothesis of the Saturnian ring, by restricting the colliding bodies to a single plane, by substituting a low initial velocity and thus rendering the moon's attraction the dominant influence, and by introducing a system of directions controlling, and therefore adjusted to, the moon's rotation, relieves the meteoric theory of its most formidable difficulty. It also explains in a simple way the abundance of colliding bodies of a different order of magnitude from ordinary meteorites and aërolites. The remainder

of my discourse will be devoted chiefly to the development of the moonlet theory, and I hope to show that it not only harmonizes with the varied details of crater character, but aids in the explanation and even in the discovery of other features of the moon's face.

The idea that a planet or satellite may be developed from a ring of matter revolving about the primary has been entertained by so many students of celestial mechanics that its introduction needs no defense. The assumption that the ring about the earth was thin and disk-like rests legitimately on the analogy of Saturn's ring. The idea that the ring, although possessed of sufficient stability to assume a definite form, nevertheless suffered some disturbance or underwent some process of evolution by which its stability was destroyed, is likewise familiar to celestial mechanics, and it does not appear necessary in this connection to speculate as to the precise manner in which the integration of its discrete elements was effected, nor does it appear necessary to assume, on the one hand, that the aggregates of ring matter constituting the larger moonlets were loosely assembled, or, on the other hand, that they were already welded into continuous masses. Provided the energy of impact with the moon sufficed to produce phenomena of fusion, the dynamic results would perhaps not differ greatly.

The velocity of impact, depending chiefly on the moon's attraction, must be supposed to have increased gradually as the moon grew. In the closing stages of the process it did not vary greatly on either side of one and one-half miles per second, and the phenomena of the present surface may be discussed on the basis of that velocity. The energy due to that velocity would more than suffice, as already stated, to melt the moonlet if it were composed of ordinary volcanic rock and provided all of the energy were applied to the heating of the moonlet. Practically only a portion of it was thus applied; another portion produced heat in the contiguous tract of the moon's material; yet another was consumed in the deformation of moonlet and moon result-

ing in the crater, and another resulted in modifications of the moon's motions, changing its orbit, its orbital velocity, its axis, and its rotational velocity. The energy converted into heat might be regarded as the remainder after deducting all other effects, and the resulting temperatures would be further conditioned by the distribution of heat in the colliding masses.

Since the area of the moon's surface directly struck by the moonlet is a function of the square of the diameter of the moonlet, while the energy applied to that area, being measured by the mass of the moonlet, is a function of the cube of its diameter, more energy would be applied to a unit of space in the case of large moonlets than in the case of small, and the temperatures caused by large moonlets would therefore be greater. To this relation I ascribe the restriction of inner plains, indicative of fusion, to the larger craters, and the same explanation applies less directly to the limited distribution of central hills.

In the production of small craters by small moonlets I conceive that the bodies in collision either were crushed or were subjected to plastic flow, and in either case were molded into cups in a manner readily illustrated by laboratory experiments with plastic materials. The material displaced in the formation of the cup was built into a rim, partly by overflow at the edges of the cup, but chiefly by outward mass movement in all directions, resulting in the uplifting of the surrounding plain into a gentle conical slope. This outward and upward movement was accentuated, possibly through the agency of heat, about the immediate edge of the cup, occasioning the special elevation called the wreath. The cups thus formed, having dimensions commensurate with the strength of the lunar material, were stable and permanent. The impact of a larger moonlet produced a larger cup, and at the same time fused a portion of the material and softened other portions. The walls of this cup were so lofty that they could not sustain their own weight, and they were further weakened by the effects of heating; consequently

they settled downward and their lower portions flowed inward toward the center of the cup. The inward flow from all sides produced at the center an upward movement, occasioning the central hill. The effect was perhaps heightened by the elastic recoil of a considerable tract of the moon's mass below and about the point of impact. At the same time the fused parts, which were partly determined by the distribution of strains and partly by the occurrence of local passages of more fusible material, flowed to the bottom of the cup, either surrounding the central hill or, if in great volume, submerging it. Sometimes minor tracts of fused matter occurred in the wreath, and the exudation of these gave rise to lava streams flowing down the outer slope. The inward flow of the lower portions of the walls undermined the upper portions, including the inner part of the wreath, so that they settled down toward and into the liquid pool of the interior, and this settling gave rise to the inner cliff and the inner terraces. In the case of some of the large craters all of the wreath was carried down.

The effect of the collision on the moonlet was not uniform throughout. The part in immediate contact with the moon, being compressed by the shock of the entire mass behind it, was probably heated more than any other part. The opposite portion of the moonlet, sustaining no blow from behind and having its motion arrested in a comparatively gradual way, was less affected and probably never fused; the results of laboratory experiment indicate that it remained central in the crater and was uplifted by the recoil so as to constitute the surface of the central hill.

The impact theory as thus developed appears competent to explain the origin of all typical features of the lunar craters. Its relation to exceptional features, as well as to associated phenomena, will presently be considered; but something should first be said with reference to certain physical factors of the process which are somewhat unfamiliar.

The production of heat by impact is a well known phenomenon, but instances in which that heat suffices to pro-

duce fusion are somewhat rare. During the earlier discussion of the doctrine of the conservation of energy Hagenbach fired a leaden bullet against an iron target,* melting a portion of the bullet. By a computation involving the velocity of impact, the weights of the melted and unmelted portions of the bullet, and the physical constants of the materials, he was able to account for more than 90 per cent. of the energy, and the theoretic relations between molar motion and heat were thus substantially verified. In ordinary rifle practice in a shooting gallery the small leaden bullets fired against the iron targets are both fused and rendered incandescent by the shock. In the course of my own experiments a small amount of fusion was produced by firing a ball of Wood's alloy against a target of the same material.

Central hills have not been produced in the laboratory by the impact of rigid materials, it being found impracticable to conduct operations on a sufficient scale, but they are readily formed with semi-liquid substances. If a drop of water be made to fall on a still surface of water the outward-moving annular wave at one instant encloses a crater; at the next instant a mound rises in the center of the crater. If for the water there be substituted a thin mud, the relations may readily be adjusted so that the viscosity of the material will arrest the motion in either phase. If the drop fall from a certain height it produces a cup-like cavity with a smooth rim; if it fall from a somewhat greater height it produces a larger cup with smooth rim and with a smooth, dome-like hill in the center (Fig. 13). Though this experiment does not yield forms closely resembling those of the moon, it serves to illustrate the process of gravitational recoil in the formation of a central hill. The peculiar conditions ascribed to the lunar phenomena, and especially the fact of local softening and fusion, seem adequate to account for the observed

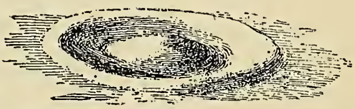

FIG. 13.—Central hill formed experimentally by gravitational recoil.

* Pogg. Ann., vol. 140, 1870, p. 486.

differences in form, but it has not been found possible to reproduce them on a small scale.

Arched Floors.—In general, the inner plains of the craters are level, except as interrupted by central hills and by craters of subsequent origin. In some of the larger it is possible to note, under favorable conditions of light and shadow, a gentle convexity corresponding to the normal curvature of the moon's surface. In a few instances, not more than a dozen in all, the convexity is seen to be greater, the central portion distinctly rising above the level of the margins. This peculiarity is most strongly marked in the floor of the crater Mersenius, which is a dome 1,500 feet high and thirty miles across. A similar but lower dome in the crater of Petavius bears on its crest a group of peaks exhibiting the ordinary characters of the central hills. Two explanations have occurred to me, each based upon the idea of a relatively soft substratum. It appears possible that deep seated matter which had been displaced horizontally by the original collision might slowly return under gravitational stress, lifting the middle of the crater floor after congelation of the impact liquid had given it a level character. It also appears possible that the strains produced by the tendency of viscous material to flow upward were not effective until reinforced by the shock associated with the formation of some later but neighboring crater. In either case the result was produced by the flow of a viscous solid analogous to the viscous movement supposed to accompany the broader terrestrial uplifts. The fact that the arch of Mersenius is cracked at the crest and that the arch of Petavius is traversed by a system of cracks is consistent, I think, with either of these explanations, but is perhaps not specially significant, as cracks occur in considerable abundance in many parts of the moon's surface.

Distribution and Overlap.—Consideration will presently be given to the fact that in certain districts craters are thickly set and elsewhere they are less numerous, but in other respects their distribution exhibits no system. They are not arranged in lines or other patterns, large and small are in-

discriminately mingled, and interference is a common phenomenon. If, as I have assumed, the moonlets approached the moon approximately in the plane of its equator, the fact is not attested by the grouping of the craters in a medial zone, and so it is necessary to assume further that the axis of rotation was not constant. This assumption need occasion no difficulty, for unless the approaching moonlets moved *precisely* in the plane of the moon's equator, their collisions would disturb its axis of rotation, and there is no reason to suppose that these disturbances would be compensatory rather than cumulative. Under the successive impulses thus given the moon's equator may have occupied successively all parts of its surface, without ever departing widely from the plane of the moon's orbit.

Sculpture.—The rims of certain craters are traversed by grooves or furrows, which arrest attention as exceptions to the general configuration. In the same neighborhood such furrows exhibit parallelism of direction. Similar furrows appear on tracts between craters, and are there associated with ridges of the same trend, some of which seem to have been added to the surface. Elsewhere groups of hills have oval forms with smooth contours and parallel axes, closely resembling the glacial deposits known as drumlins, but on a much larger scale. Tracing out these sculptured areas and platting the trend lines on a chart of the moon, I was soon able to recognize a system in their arrangement, and this led to the detection of fainter evidences of sculpture in yet other tracts. The trend lines converge toward a point near the middle of the plain called Mare Imbrium, although none of them enter that plain. Associated with the sculpture lines is a peculiar softening of the minute surface configuration, as though a layer of semi-liquid matter had been overspread, and such I believe to be the fact; the deposit has obliterated the smaller craters and partially filled some of the larger. These and allied facts, taken together, indicate that a collision of exceptional importance occurred in the Mare Imbrium, and that one of its results was the violent dispersion in all directions

of a deluge of material—solid, pasty, and liquid. Toward the southwest the deluge reached nearly to the crater Theophilus, a distance of 900 or 1,000 miles, and southward it extended nearly to the latitude of Thebit. Northward and northeastward it probably extended to the limb. Westward it passed beyond Posidonius, and toward the east and southeast its traces are lost in the Oceanus Procellarum. Its more liquid portion gathered on the lowlands, giving rise to several maria and minor plains. The fact has been recognized by various students, notably by Green * and Meydenbauer,† that many of the lunar plains are due to floods of molten material overspreading the low lying tracts and burying the preëxistent irregularities of surface. At various points in such plains, and especially at their margins, crescentic hills project above them, recognized as portions of crater rims; and elsewhere the plains are divided by systems of cracks whose arrangement betrays the distribution of underlying ridges. The plains most closely associated with the sculpture system and the supposed viscous deposit are the Sinus Roris, Mare Frigoris, Lacus Mortis, Lacus Somniorum, Palus Nebularum, Mare Tranquilitatis, Mare Vaporum, Sinus Medii, Sinus Estuum, and Mare Nubium. The Oceanus Procellarum may have been created at the same time or may have been merely modified by this flood. The Mare Serenitatis, whose sharp outlines and circular form mark it as an old crater, doubtless received a new surface.

As to the precise nature of this catastrophe I am in doubt. Its focus lies within the great crater rim of the Caucasus, Apennine, and Carpathian ranges, but is not concentric with that rim, and it is not surrounded by a rim of its own. The lofty plateau lying north of the Mare Imbrium, although presenting a steep face toward the mare and a long slope in the opposite direction, has not the simple contour of a crater wall, but is variously notched. By considering the extent and probable thickness of the various deposits from the

* E. N. Green: Jour. Brit. Ast. Ass., April, 1891, p. 379.
† A. Meydenbauer: Sirius, February, 1882.

flood, it has been estimated that its volume may have equaled a sphere 80 or 100 miles in diameter, and there is perhaps no occasion for surprise that the results of the collision of a body of such magnitude were exceptional in character as well as extent.

So far as I am aware, these features of sculpture have not previously been recognized.* It is therefore of special importance that my observations be verified by those of others,

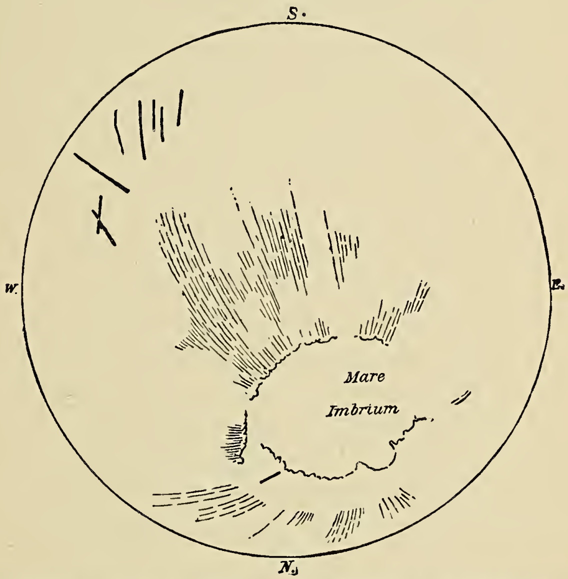

FIG. 14.—Trends of lunar sculpture. General sculpture is represented by shading; great furrows by heavy lines. Irregular lines show crests of uplands surrounding M. Imbrium.

and to this end the general statement will be supplemented by the enumeration of enough particulars to serve as a clue to the recognition of the novel phenomena. The general distribution of the sculpture, comprising the districts in which it is faintly exhibited as well as those in which it is conspicuous, is indicated in Fig. 14, where the shaded areas

* Parallelism has been noted by Beer and Mädler in the tract south of M. Serenitatis (Der Mond., p. 250), and by Neison south of the Carpathians (The Moon, p. 309), but no reference is made to sculpture.

represent sculpture districts and the direction of the fine parallel lines indicates the trend of the sculpture. The interspaces between shaded areas are largely occupied by maria and other plains on which no sculpture appears.

The boldest carving is seen on the Apennines, the crest line of which is cut into battlements. From the hollows between battlements, rude grooves follow radially down the southern slope to its base. A similar sculpture appears on the Caucasus, but the range is traversed obliquely, from E. S. E. to W. N. W. The sculpture features of the Carpathians are less conspicuous, but immediately south of them is a tract occupied by drumlin-like hills, the axes of which point toward the Imbrian plain. The broad isthmus between the Mare Vaporum and the Mare Serenitatis is so thoroughly sculptured that most of its features exhibit parallel trend in a southwesterly direction. The great crater of Julius Cæsar has lost most of its southern wall, and its valley is filled to the level of the remnant of rim on that side. Boscovich is barely to be recognized as a crater, and whatever other craters may have antedated the flood are defaced beyond recognition. Manilius, Menelaus, and a few others are of more recent date, and their clean cut features stand in striking contrast to the general ruin. Similar features with slightly different trend characterize the plain for 200 miles south of Julius Cæsar. The rims of Hipparchus, Albategnius, Ptolemy, and Alphonsus are all notched by grooves trending toward the Mare Imbrium, and some of these grooves can be traced to the vicinity of the crater Lalande. The rims of Parry, Bonpland, and Guerike, jutting islandwise from the Mare Nubium, are similarly notched, the trend here coinciding almost precisely with the meridian. Through the entire region lying between the Mare Nubium and the Maria Serenitatis and Tranquilitatis, sculpture and the associated veneering have so modified the surface that there is no difficulty in discriminating the craters of later date from those of earlier. The whole topography may be classified as antediluvial and postdiluvial. The only small

craters are those of the later series, as the older have been filled and buried. Craters of the older series have lost in accentuation not only through the paring of their rims but also through the partial filling of their valleys, and their rims no longer exhibit the fine details of inner terracing and outer furrowing. Compare in these respects Hipparchus, Albategnius, and Alphonsus of the old regime with Alpetragius, Horrox, Theophilus, and Copernicus of the new.

Adjoining this district on the south and extending thence to the south pole is a broad area, known as the honeycomb district, to which the flood did not extend and with which the characters of the flooded district may be compared. In the honeycomb district distinctions of age may, indeed, be recognized, but there is gradation instead of sharp demarcation between old and new. Those parts of the surface which have been longest exempt from the downfall of large bodies are profusely pitted with minor craters, and it is these which dim the outlines of larger formations of ancient date.

Thus, by the outrush from the Mare Imbrium were introduced the elements necessary to a broad classification of the lunar surface. A part was buried by liquid matter whose congelation produced smooth plains. Another part was overrun by a flood of solid and pasty matter which sculptured and disguised its former details. The remainder was untouched, and probably represents the general condition of the surface previous to the Imbrian event.

Furrows.—In strong contrast with all other features of the moon's surface are a series of gigantic furrows. In general direction they are remarkably straight, but their sides and bottoms, with a single exception, are jagged, abounding in acute salients and reëntrants. If one thinks only of their apparent size instead of their real magnitude as he examines them through the telescope, he is reminded of the rude grooves sometimes seen on glaciated surfaces where the corner of a hard boulder, dragged forward by the ice, has plowed its way through a brittle rock. Despite the enormous disparity in size—a disparity no less than that of a mountain

to a molehill—I believe that this resemblance is more than accidental, and that the lunar furrows were really formed by the forceful movement of a hard body; but the graving tool in this case, instead of being slowly pushed forward by a matrix of ice, moved with high velocity and was controlled only by its own inertia. It was my first idea that the furrows are the tracks left by solid moonlets whose orbits at the instant of collision were nearly tangent to the surface of the moon, and for some of them I have still no better explanation to suggest; but when they came to be platted on a chart of the moon's face it was found that more than half of them accord in direction with the trend lines of the Imbrian outrush, a relation which can be seen in Fig. 14, where they are represented by heavy lines. It thus appears possible, if not probable, that they were produced simultaneously with the Imbrian deluge, and the implication of power is thereby rendered even more impressive. What must have been the violence of a collision whose scattered fragments, after a trajectory of more than a thousand miles, scored valleys comparable in magnitude with the Grand Cañon of the Colorado!

So far as I am aware, but four of these furrows have been previously recorded, and only two are well known, the valley near Rheita and the valley of the Alps, and I therefore invite the attention of observers to the localities indicated by the following descriptions. Beer and Mädler, who set forth the character of the Rheita valley at length and with great clearness, state that it is 187 miles long and from 10 to 25 miles broad and has a maximum depth of more than 11,000 feet.* Its general course runs from the eastern tangent of the crater Rheita southwestward to the northern margin of the crater Rheita *d*. Not far from its southwestern extremity starts a smaller valley with a somewhat more southerly course. Parallel to this latter valley and somewhat to the southeast are a number of minor grooves which give a striated appear-

* Der Mond, page 389.

ance to an area between Metius and Vlacq. One of these can be traced northeastward to a point just west of the crater Piccolomini. Although unquestionably a unit, it is not continuous, but appears here and there as though the projectile grazed only the higher uplands, and it is locally blotted out by the craters Metius and Fabricius, which are of more recent date. Its total length is 450 miles. Another of the same system appears to the northwest of the crater Furnerius. More conspicuous than these and more westerly in trend is a groove seen first at the southern tangent of Borda and traced for 500 miles west-southwest across the southern part of Snellius and past the southern margins of Haze and Adams. Its width is in general about 10 miles, but width and depth are irregular, and it leaps a number of valleys, including the bed of Snellius. Beer and Mädler * record a part of it just west of Snellius, but apparently saw no other part. West of the Mare Nectaris and following the eastern base of the Pyrenees mountains is a trough which should probably be classed with the furrow system, but I have not seen it with the illumination suitable for the determination of its details. In length and width it resembles the Rheita valley, and it trends nearly with the meridian. It is crossed obliquely by a narrower groove trending approximately with the Imbrian system and intersecting the crater Capella. This is partly represented on Schmidt's map. My notes indicate also a very old furrow trending southeastward from a point about 175 miles east from Clavius, but its precise position was not determined. The straight valley traversing the lunar Alps, better known than any of the others, is likewise exceptional in character. Its sides are no less irregular than those of the other furrows and are even more precipitous, but its bottom is smooth, so that it constitutes a comparatively narrow, flat-bottomed defile, traversing the plateau from side to side. Neison gives its length as 83 miles and its breadth as ranging from 6 to $3\frac{1}{2}$ miles, but at

* Der Mond, page 375.

a point near its southern end it is still narrower. Its maximum depth is more than 10,000 feet. Its trend coincides with that of the Imbrian sculpture in its vicinity, and it thus helps to unite the furrow group of the western district with the great sculpture system. The flatness of its bottom is readily explained as a result of partial flooding, but its constriction is less easily explained.*

Wargentin.—One of the most striking anomalies of the moon's face is the plateau Wargentin. It is a smooth, circular table, 54 miles wide, standing several thousand feet above its base, and bearing a low parapet about the greater part of its edge. It is readily recognized as a crater that has been filled by molten rock to the level of the lowest point of its lofty rim, and the determination of its mode of filling is a problem that has occupied the attention of all selenologists. A solution consonant with the moonlet theory is suggested by the Imbrian deluge. As already noted, the crater of Julius Cæsar is filled as high as the breach through its southwestern rim. The filling, however, was not even, as the added material was not sufficiently fluent to acquire a level surface. Five hundred miles away, in a district where the deluge was more liquid, the crater of Posidonius shows a level floor at the height of the lowest point of the rim. It happens that the walls of Posidonius are very uneven, and that their lowest pass is only a little higher than the neighboring Mare Serenitatis. Its floor therefore does not attract attention as an elevated plateau, but the relation of floor to rim is essentially the same as in Wargentin, and community of origin is a natural inference. Further study will be required to determine the source of the Wargentin accession, but clues are not wanting, for the neighborhood abounds in evidence of flooding. Close to Wargentin's western base lie three craters, of which the nearest, Phocylides *b*, is partly

* While engaged in telescopic study I did not realize the importance of close attention to the details of this district, and these pages will go to press before I have opportunity for renewed observation.

filled and Phocylides *c* is almost completely filled, though the largest, Phocylides, is empty. To the northwest of these and to the south of Mare Humorum is a broad tract characterized by much filling of craters and by the obliteration of minute craters. It has the general aspect of the Imbrian region, but I have not ventured to include it in the Imbrian chart, as it is separated by nearly 200 miles of the Nubian plain from the nearest district distinctly sculptured. If it did derive its overwash from the Mare Imbrium, then that flood extended in this direction more than 1,500 miles, and must have swept over the entire Oceanus Procellarum. Be this as it may, Wargentin and the neighboring lowland probably have a common flood history.

Rills and Rill Pits.—Among the most difficult of the moon's enigmas is the problem of the "rills." Narrow defiles, often tapering at either end, they suggest fissures; but fissures taper downward also, and many of the rills have flat bottoms. Deep canyons, with parallel steep walls, they suggest stream beds; but stream beds have continuous descent in one direction, and the rills run up hill as well as down.

Here again the Imbrian deluge affords a clue. Close by Julius Cæsar, and in the same district of pasty—or, at least, non-liquid—overwash, lie the great rills of Ariadæus and Hyginus. The rounding of their edges marks them as antediluvial features; what was their condition before the flood? Certainly not the same as now, or they would have been filled and obliterated. I imagine them as yawning chasms three-fourths of a mile wide at top and several miles in depth. As the swift tide rushed over them a small portion may have been arrested and engulfed, but the chasms were not filled until the torrent stopped. Then that which spanned them sank down, coming to rest a short distance below the edges, and so forming the visible floors. The pits that interrupt these floors are definitely related to the rills and cannot be classed as impact craters. Possibly here and there an arch of debris that had clogged the

crevice gave way, letting what lay above it pour into the abyss below. Possibly there was moisture in the crevice, and the inrush brought heat enough to cause explosions of steam. If the floor fell in, the pits should be rimless; if it was blown out, they should be rimmed. I was not able to satisfy myself as to their actual character, and recorded observations are discrepant, but the features are not too minute for accurate determination.

In a part of the rill of Hyginus the pits are set so close that their edges adjoin; other rills are composed wholly of pits, and these lead by gradation of characters to rows of separate pits where no rills are visible. Fine illustrations of the last may be seen along the western base of Copernicus, almost half way to Eratosthenes. If my conjecture is correct, these mark the line of a fissure that was filled by a molten flow connected with the formation of Copernicus.

The rills that have no visible bottoms, but are seen only as black lines, are the unfilled fissures necessary to complete this series of features at the opposite end.

White Streaks.—The only remaining great group of features are the white streaks. These are bands of color, sometimes faint, sometimes brilliant, but always indefinitely outlined, like the tail of a comet, and some of them stretch for long distances across the moon's surface. Their courses are independent of the configuration. They pass up and down the slopes of craters without either modifying their forms or being interrupted by them. The more prominent of them, and probably all, occur in systems, and those of each system radiate from some crater. This crater is itself lined with white and is usually more resplendent than the radiating streaks. We need not take time to consider the various conjectures which have been published concerning their origin. It suffices to say that all but the least plausible of these conjectures have been advanced as suggestions merely and have not been fully endorsed by their authors; but there is an unpublished suggestion, made by Mr. William Würdemann, of Washington, which is at once so apt and so simple that I

am confident it contains the essence of the theory that will finally be adopted. Mr. Würdemann is one of a number of students who have independently advocated the meteoric origin of the lunar craters, but his views have never been published. In a letter on the origin of the lunar topography, addressed by him to Dr. B. A. Gould, occurs the following passage: "The most remarkable appearance on the moon, for which nothing on earth furnishes an example, is presented by those immense radiations from a few of the larger craters—perfectly straight lines, as though marked with chalk along a ruler—starting from the center of the crater and extending to great distances over every obstruction. My explanation is that a meteorite, striking the moon with great force, spattered some whitish matter in various directions. Since gravitation is much feebler on the moon than with us and atmospheric obstruction of consequence does not exist, the great distance to which the matter flew is easily accounted for."

This explanation appeals strongly to the eye. The ray systems resemble splashes so closely that it is difficult to understand why the idea that they really are splashes has not sooner found its way into the moon's literature. It accounts for the straightness of the rays, for their vanishing edges and ends, for their independence of topography, for their relation to craters, for the whiteness of the associated craters, and for the nimbus in which the rays sometimes unite close to the crater. It explains the white crests of many gray craters, for peaks would intercept more than their pro rata of the horizontal shower.

It raises also a number of questions, the discussion of which should throw much new light on the moon's history. What is the white substance? Why do its traces become faint in passing from the bright uplands to the dark plains? Why do wavy lines replace straight ones in the radiation from Copernicus? Why do certain great rays of Tycho's system trend toward a point on the rim and not toward the center of the crater? Why are several craters, especially

Tycho, surrounded by a relatively dark band inside the bright nimbus?

As no dark rays emanate from the dark craters, it may be inferred that the white substance is peculiar in its tendency to fly about at the instant of collision. It is probably a readily fusible solid. If the ring of moonlets and the earth were once the outer and inner parts of the same whirling mass, the moonlets should consist of substances somewhat abundant in the earth's crust. Inquiry may therefore be addressed to familiar fusible substances of pale color. Fusibility in this case is measured by a factor involving the initial temperature, the melting temperature, the specific heat, and the latent heat of fusion; and this must be compared with the amount of energy expended in collision. In the short table I have constructed, the energy necessary to melt a unit mass of a substance is expressed as a fraction of the energy of motion destroyed by the collision of a unit mass. The ratios were computed on the postulates, first, of an initial temperature of —273° C.; second, of an initial temperature of —100° C. Ratios are added for the rock diabase, as a possible representative of the less fusible moonlets.

Substance.	Relative energy for fusion from —273°.	Relative energy for fusion from —100°.
Tin	.06	.05
Phosphorus	.11	.05
Sulphur	.11	.07
Silver	.15	.13
Nickel*	.30+	.27+
Ice	.33	.19
Calcium sulphate*	.43+	.38+
Sodium chloride	.46	.40
Diabase	.69	.61

Attention is naturally directed to ice by reason of its abundance on the earth and its whiteness. If it exists on the moon as a solid or liquid, it must also exist as a gas, for it would evaporate until the resulting atmosphere had a

* Latent heat of fusion not known, and ratio therefore too small.

certain pressure definitely related to the temperature of the ice. The low temperatures ascribed to the moon by Langley would correspond to an atmosphere of aqueous vapor so tenuous as to be very difficult of recognition; so that the prevalent doubt as to the demonstration of a lunar atmosphere need not bar speculation as to lunar ice. The atmospheric pressure which W. C. Pickering estimates as possible* would indicate a maximum ice temperature of about —40°. The question whether a moonlet could consist partly or wholly of ice is more vital and more difficult.

Tin, silver, phosphorus, and sulphur are more easily fused than ice and their physical properties are perhaps equally adjusted to the requirements of the problem; but tin and silver are rare substances, while phosphorus, which is less rare, does not occur naturally uncombined, and sulphur, though abundant in combination, is rare in the free state. Perhaps the free iron and nickel of aërolites may stand sponsor for free sulphur or phosphorus in moonlets.

The white bands grouped about Copernicus, though unmistakably derived from that center, do not radiate directly from the crater, are not straight, and are not of even width. They appear also to be diverted by the crater Eratosthenes, passing beyond it on both sides, but leaving a free space in its lee. These characters, and the rill pits previously described, lead me to refer the bands to a swift liquid flow over the surface. The flow probably included two substances, the darker of which, occupying interspaces between the pale bands, is not distinguishable in color from the surrounding maria. The straight feathery rays from other craters are referred, in contradistinction, to jets or sprays projected free from the surface.

Retrospect.—In the preparation of this manuscript I have been gradually drawn from the attitude of the judge to the attitude of the advocate. This transformation is but an echo of the history of the investigation, for, starting with two working hypotheses, the impact and the volcanic, I soon

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* Astronomy and Astro-physics, 1892, p. 778.

found that the details of crater forms accorded so perfectly with my theoretic conception of the results of impact and so imperfectly with the results of volcanic action that further consideration of volcanic possibilities was unprofitable. Brief examination of other theories, as they were discovered one by one in the literature of the subject, satisfied me that they could not compete with the impact theory as interpreters of crater form, and thus I was led to devote myself to the development of the impact theory. As one after another the obstacles in its path were found to be movable, and as one after another the obscure phenomena of the moon were found to be illuminated by its light, it gradually ceased to be viewed as a tentative explanation and was adopted as the real explanation. The tendency toward advocacy thus sprang from conviction, but it has been further promoted by the consciousness that there are many in my audience who do not share with the student of geophysics his conception of the plasticity of rock masses. Our every day experience tells us that rock is brittle, and the correlative fact of its viscosity is not practically accepted on the mere dictum of the physicist and the geologist, unless their paths of approach are to some extent retrod. So results of impact which seem to me entirely natural are to some of you extravagant and inconceivable; and if the impact hypothesis is to abide with you, it must ingratiate itself by an attractive array of accomplishments.

The analytic examination of volcanic processes left the possibility that the small craters of the moon are maars, the results of explosion without eruption of lava; the tidal process might perhaps make large craters, but could not make small ones. These are the only suggested reactions originating in the moon itself which appear competent to produce the crater forms actually observed. Taken together, they cover all the craters, but they cannot be applied as a joint theory without arbitrarily dividing a series the gradation of which is complete as to both size and form. The impact theory applies a single process to the entire series

(excepting only the rill pits), correlating size variation with form variation in a rational way. Specialized by the assumption of an antecedent ring of moonlets, it accounts also for the great size of many craters. It brings to light the history of a great cataclysm, whose results include the remodeling of vast areas, the flooding of crater cups, the formation of irregular maria, and the conversion of mere cracks to rills with flat bottoms. It explains the straight valleys and the white streaks. In fine, it unites and organizes as a rational and coherent whole the varied strange appearances whose assemblage on our neighbor's face cannot have been fortuitous.

Growth of the Moon.—In an incidental way there has sprung from this investigation of the moon's craters a theory as to the building of the moon itself. An attempt to develop that theory would lead far afield, but it is due to the crater discussion that its implications as to the moon's history be brought together, so that their coherency may be judged.

In the breaking up of the postulated pre-lunar ring there were at first many centers of aggregation,—were the moon the only center, the scars of impact would all be small. So long as the masses were small the process of aggregation developed little heat, for the heat of impact depended almost wholly on velocities created by mutual attractions. That particular moonlet which became the nucleus of the moon may therefore be conceived as cold, or at least as sufficiently cool to be solid. As the moon's mass grew, the blows it received were progressively harder, and for a time their frequency also increased. The rate of heating probably reached and passed its maximum while the mass was materially less than now. During the whole period of growth the surface lost heat by radiation, but the process of growth cannot have been slow enough to permit the concurrent dissipation of all the impact heat. On the one hand, there should have been some storage of heat in the interior, and, on the other hand, the stored heat can never have sufficed for the liquefaction of the nucleus. Toward the close of the process, when blows

were hard but rare, liquefaction was a local and temporary surface phenomenon, but the general temperature of the surface was low. Impact heat, being evolved simultaneously in the surface and the subsurface, was dissipated more rapidly from the surface, so that there was a subsurface zone of relatively high temperature. The zone thus inferred deductively is also inferred inductively from the disparity of cavities and rims in the case of large craters (page 260); but, on the other hand, there is little evidence of the wrinkling which, theoretically, should result from the adjustment of a cold crust to a cooling nucleus. The parallel topography southeast of Mare Serenitatis is due to sculpture, and not to buckling. The Apennine range, sometimes described as a wrinkle, is part of a crater rim. The great cliff called Altai mountains probably marks a fault, but has not the habit of a range lifted by tangential thrust. The only indubitable flexures that may be ascribed to crustal adjustment traverse the maria, whose smooth floors are admirably adapted to their display. They have anticlinal and monoclinal forms, but are so gentle of slope that they are seen only near the terminator, and can represent but a minute amount of arc shortening. It is therefore probable that the final shrinkage of nucleus was small, and the antecedent storage of heat correspondingly small. During the whole period of growth the body of the moon was cold.

This sketch of the life of our nearest neighbor has but little in common with the accounts of other biographers. To her has been ascribed a fiery youth, after the manner of the sun, a middle life of dissipation, like Jupiter and Saturn, a hardening and wrinkling old age, toward which the earth is tending, and, finally, the end of change, death. If the record of her scarred face has now been read aright, all that remains of the old narrative is its denouement: the moon is dead.

Age of the Moon.—Selenographers are not yet satisfied that the condition of the lunar surface is constant, although the history of their search for changes is discouraging. If

the moon's face shall prove absolutely incommunicative of modern change, it cannot be expected to reveal the date when its expression last was varied; but, strange as it may seem, the earth can give a partial answer, for the earth was an actor as well as a spectator of the moon's drama, and the record of its participation lies somewhere among the archives of its crust. While the moon was growing the relations of orbits and attractions were such that any moonlet which narrowly escaped collision with the moon was enormously perturbed, acquiring an entirely different orbit about the earth. Many must have been so directed as to collide with the earth, and the traces of their collision, if ever discovered, will tie together at a new point the chronologies of satellite and planet.

The results of collision with the earth may have been very different from the lunar phenomena. The energy of impact determined by the earth's attraction was 22 times as great as that determined by the moon's. It would suffice to melt a body of diabase 30 times as large as the moonlet. The shock was somewhat lessened by the atmospheric cushion, but a moonlet of medium size must have developed an immense quantity of heat, and may be imagined to have projected molten rock far and wide, just as the white streaks were projected over the moon.

Does the earth exhibit impact craters? If not, then erosion and sedimentation have destroyed them, and the Cenozoic era did not witness the building of the moon. Is any horizon of stratified rocks generally or widely characterized by molten disjecta? If not, then the moon was already a finished planet in Paleozoic time. Should both questions be answered in the negative, and the lunar event thus relegated to the hazy dawn of the geologic day, is it then possible that the earth, by taking tribute from the moonlet swarm, introduced into its crust an element of heterogeneity, which initiated not only the differentiation of continental and oceanic plateaus, but the series of geographic transformations of which geologic structure is the record?

Acknowledgments.—Before my final bow is made it is due that a moment be given to recognition of the facilities and aid I have received in the conduct of the investigation. Through the courtesy of Captain McNair, superintendent of the Naval Observatory, and with the cordial coöperation of the astronomers of his staff, I have been enabled to study the lineaments of the moon's face through the great Washington equatorial. The kindness of Professor Rood and Professor Hallock accorded me ample space and facilities for experimentation in the Physical Laboratory of Columbia College. The courtesy of Professor Langley, Secretary of the Smithsonian Institution, and the generosity of Professor Hale, of Chicago, secured for my study fine series of lunar negatives and photographs.

The progress of my work has carried me in many directions beyond my proper field of physiography into the fields of physics and celestial mechanics, and in these unfamiliar ways my hesitating feet have been guarded and guided by certain masters in those fields whom I am no less fortunate than proud to number among my friends. I refer to Professor Newton, of Yale College, and to our fellow-members, Dr. Barus, Professor Abbe, and Professor Woodward.

THE TEXAN MONSOONS.

BY

MARK WALROD HARRINGTON.

[Read before the Society, January 6, 1894.]

By seasonal winds is meant those which continue in the same direction for periods which are measured by months and recur in the same seasons. The ordinary charts giving the resultant or average direction do not admit of safe conclusions on such winds. On the contrary, the tendency is to eliminate them. For instance, if at any station the wind was generally north for one-half of the year and south for the other half, except as modified by cyclonic or anti-cyclonic action, the last would, though exceptional, give character to the charts, and the more interesting periodic wind might be completely eliminated.

In order to ascertain the locality and character of annually recurring winds of considerable duration, maps were constructed showing the most frequent winds for each month of the year. The tables employed were the manuscript ones of the Weather Bureau, which give the mean monthly frequency for the eight principal points of the compass for the tri-daily observations from 1871 to 1886, inclusive. The tri-daily observations were used because they would give less prominence to land and sea breezes than would the later bi-daily observations. On these charts were marked all winds occurring with a frequency of two-eighths of all the observations or more, and the same for three-eighths, four-eighths, and five-eighths. Of the latter there were only three cases. Alaska was, of course, omitted, as we have few data from that region.

The completed charts show the stations where for any month the wind was twice, thrice, four, or five times as frequent in any direction as would be the case if the winds were uniformly distributed. This was taken to indicate the most frequent winds and to point out the regions deserving more detailed study. Inspection of the maps showed the following cases where there was a distinct seasonal change in wind direction:

I. The Texan monsoons, extending up the plains sometimes beyond the limits of Texas and even to the northern boundary of the United States.

II. The California monsoons, occurring on the Pacific coast south of San Francisco and reaching up the Sacramento and San Joaquin valleys.

III. The Willamette and Puget sound periodic winds.

IV. The northeast and southwest winds of the Atlantic coast.

V. The off-coast winds in winter above cape Henry.

VI. Isolated cases, as Escanaba, Duluth, Las Animas, and Winnemucca.

Aside from these, the direction of greatest frequency of the wind is generally westerly over the entire country. In the Great Basin the indications are somewhat ambiguous. Over the great lakes it is generally west or southwest. Over the more northern part of the Rocky Mountain region it is west or northwest, while in the southern part (Arizona) it is westerly, but there seems to be no decided preponderance of the northwest winds over the southwest ones.

Several of these winds are designated as monsoons. A monsoon is, so to speak, an annual land and sea breeze. It is a wind that changes its direction twice a year because of the differences in temperature of the continental and oceanic surfaces over which it flows. In summer the land surface is the warmer and deflects the wind toward itself. In winter the oceanic surface is the warmer and deflects the wind in its direction. It is not necessary that there be a complete reversal of direction, though this happens in the

case of the best known monsoons—those of southeastern Asia. The monsoon can also be defined in terms of the atmospheric pressure. The wind flows from the region of higher to that of lower pressure. In the case of large adjacent areas of land and water (especially if the land is bare of forests, and still more if it is a desert tract) the higher pressure is over the land in winter and over the ocean in summer. The wind is in this case alternately deflected toward the land and the ocean, giving rise to a monsoon. As the relatively high temperature of one and the low temperature of the other is the cause of the differences of pressure, this conception is really only another form of the preceding.

The occurrence of monsoonal winds on the North American continent has been but little discussed. As early as 1853 Mr. Lorin Blodget called attention to the summer southeast wind of central Texas and called it a true monsoon,* and in 1875 Professor Coffin discussed † the monsoonal features of the United States in general terms. General Greely, dissenting from these views, says that he "cannot agree with those who credit the United States with monsoonal winds." ‡ This may be considered by him a question of definition rather than a question of fact, as he says the attempt to apply the name monsoon to wind systems of other regions than those of southern Asia has not gained general consent. With this Professor Ferrel does not agree, for on his discussion of monsoons are founded the definitions above given. Besides, to extend the name of a typical phenomenon to all the phenomena of the same type is a common custom and a very convenient one. Professor Ferrel points out § that to have well marked monsoons the continental area must

* Amer. Assoc. Adv. Sci. Proceedings, 7th Meeting, held in Cleveland, Ohio, July, 1853, 8°, Cambridge, 1856 [vol. 7], p. 177.

† Coffin (J. H.). Winds of the globe, being Smithsonian contributions to knowledge; vol. 20, fol., Washington, 1875.

‡ Greely (General A. W.). American weather. 8°, New York [1888], p. 164.

§ Ferrel (William). Popular treatise on the winds. 8°, New York, 1890, chapter v, pp. 193–226.

have elevated plateaus or be backed by high mountains. These elevated regions serve something like a chimney flue for the inflowing air when they are heated, and increase the velocity of the outflowing air when chilled. It is a requirement of prime importance and is overlooked by most writers on the subject. It is a condition which exists in the United States, and its influence can be distinctly seen in each of the cases of marked monsoons mentioned. Indeed, Professor Ferrel, with a rare combination of both analytic and synthetic powers, not only analyzed the monsoon as a type and pointed out its essential features, but he also indicated the parts of the earth's surface on which monsoons may be found. He apparently depended for this, not on the examination of the weather maps and monthly charts, but on general principles. Nearly every one of the special winds mentioned here were indicated by him in general terms. The only work with regard to them that he left to do was to fill in the details of the pictures which he had sketched in general outline. For the Texan monsoons such details will now be given as can be drawn from the daily weather maps and from the study of the monthly maps of greatest frequency already referred to.

They are in-coast and off-coast winds, and are south and southeast, north and northwest on the coast and in Texas, but become southerly or northerly winds when they extend far up on the plains. Up toward the Dominion border they pass imperceptibly into the prevailing southwesterly and northwesterly winds. A separate discussion of the southerly (summer) and northerly (winter) yields the clearest understanding of them.

(*a*.) The Southerly Winds or Summer Monsoon.

These first appear distinctly in March, when they occupy the territory south of a line drawn from Texarkana to the mouth of the Pecos river. Their eastern limit is not well defined, either in this or the succeeding months.

In April the territory occupied by them is somewhat

larger. It now lies to the southeast of a line drawn from Fort Sill, in the southwestern part of Indian Territory, to the mouth of the Pecos.

In May it extends to a meridian perhaps fifty miles westward, and swells northward until it reaches the Dakotas, Minnesota, and Wisconsin—in fine, it extends fairly to the Dominion boundary. From June to September it remains about the same. In October it is as in May, and in November it disappears.

It is most extensive from May to October, when it occupies a territory ten degrees of longitude (about 500 miles) wide and fifteen degrees of latitude (about 1,000 miles) long.

The examination of the daily weather maps for the year 1891 and for fractions of other years brings out several noteworthy features in the relation of these southerly winds to cyclones and anti-cyclones. This northward draft of air between the Rocky mountains and the Mississippi river is frequently disturbed and modified by these independent wind systems. Sometimes the reach of the southerly winds is increased thereby; sometimes it is decreased; sometimes it is suppressed, especially in the earlier and later months. Very rarely, however, are they entirely suppressed during the summer months. They are apt to persist through all weather along the coast, especially west of Galveston, and they show an invincible determination to extend themselves northward through every gap where they can force their way. They frequently overcome the winds in the southwest quadrant of a cyclone lying immediately northeast (when the cyclonic winds are from the north), and in more than one case they were found pouring through between cyclone and anti-cyclone when the currents of both these systems were against them. For the first, see the weather maps for May 18th, 26th, p. m., and June 7th, p. m.; for the last, see June 20th, all in 1891.

But the wind systems of the cyclone and anti-cyclone are not always unfavorable. Sometimes they act in the same direction as the monsoon, in which case they extend it northward until it may reach beyond our northern frontier.

An examination of the individual effects of cyclones and anti-cyclones, resulting from their geographical position, brings out several results, some of which are unexpected. Cyclones west of the continental divide have no appreciable effect. As they pass over the divide north of Texas they promote the northward extension of the southerly winds until the center passes the meridian of eastern Texas. After that they tend to suppress the southerly winds, but they generally have much less influence when retreating than when advancing. When a cyclone passes along Texan latitudes and over that State there is less effect than might be expected. When a cyclone forms in Texas or in the adjacent part of the Gulf, the effect on the southerly winds is very marked. This is especially true in the initial stages of a cyclone. In such cases the Texan winds are in confusion, and it is only by examining subsequent maps that the causes are manifest. As soon as the development of the cyclone has reached such a stage that the isobars and isotherms are irregular and bent, but before a distinct area of low pressure can be detected, the southerly winds yield the struggle and the wind vanes set to all directions.

The anti-cyclone has decidedly more effect than the cyclone, as might be expected, seeing that it reverses the slope of the isobaric surfaces. An interesting fact is that the anti-cyclone effect on the Texan southerly winds is more pronounced when it is east of the Mississippi river than when west of it. An anti-cyclone over the upper lakes has greater effect than one culminating in the Dakotas. One central in Tennessee or even in Alabama will have, while one off the Georgian coast may have, a marked effect. Even from Ontario it may have greater effect than to the westward.* On the a. m. map of July 22, 1891, an anti-cyclone over Maine appreciably disturbed the southerly winds in Texas.

When an anti-cyclone is over or near Texas it controls the winds generally. It may set them only one or two points away from the north or it may reverse them.

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*See War Department weather map for June 20, 1891, and the days immediately following.

The weather maps are now based on observations taken at 8 a. m. and 8 p. m. of Eastern standard time. This is about 6 a. m. and 6 p. m. by Texas local time. An inspection of the maps shows that the disturbances of the southerly winds caused by cyclones and anti-cyclones are appreciably greater in the early morning than in the early evening. This is probably due to the fact that the temperature is at about its minimum at 6 a. m. in summer, and a wind due to differences in temperature of land and water would be weaker at that time than at 6 p. m.—a few hours after the daily maximum of temperature.

To make a thorough investigation of the velocity of the southerly winds would require a collation of the records by hours and days for the winds from different directions. This is an arrangement of the data which is not usually made, and it has not as yet been made in the Weather Bureau. It would require a great deal of labor to make it. The record sheets of the registering anemometers show, on glancing through them and taking only the cases where the southerly winds are fairly independent of cyclonic or anti-cyclonic action, that these winds often continue day and night, but with a decrease of velocity in the morning hours. As a typical case, the record at Abilene, which is far inland, for the dates August 13 to August 21, 1891, shows that the mean velocities for the south and southeast winds during this time were as follows:

Wind Velocities at Abilene, Texas, August 13 *to August* 21, 1891.

Hours.	Miles per hour.	Hours.	Miles per hour.	Hours.	Miles per hour.	Hours.	Miles per hour.
Midn....	9.7	6 a. m.	9.6	Noon...	11.8	6 p. m.	13.8
1 a. m...	9.1	7 " ..	8.1	1 p. m.	10.5	7 " ..	13.9
2 " ...	9.3	8 " ..	9.7	2 " ..	12.2	8 " ..	11.0
3 " ...	9.6	9 " ..	12.2	3 " ..	12.7	9 " ..	10.1
4 " ...	9.2	10 " ..	12.9	4 " ..	13.5	10 " ..	10.6
5 " ...	9.8	11 " ..	11.9	5 " ..	12.6	11 " ..	10.4

Dr. I. M. Cline, of Galveston, states that "the south winds in midsummer in the central portion of the State do not, under ordinary circumstances, change during the night, but continue from the south, although with less velocity early in the morning than at other times during the day."

The summer monsoon winds in Texas are somewhat gentler than the winter ones, but this is generally true for winds in temperate latitudes. High winds in Texas in summer are usually cyclonic ones.

Another feature of interest shown by an examination of the weather maps is that these winds usually give clear weather except when cyclonic conditions approach. They then give a fair or overcast sky and cause an extension toward the south of the cyclonic cloud-cap. Occasionally, under these circumstances, isolated patches of rain may appear.

Dr. Cline points out a secondary or resultant condition to the west of the monsoon region. During the summer monsoons, in dry seasons, to the west of the meridian of 98°, there is occasionally a hot southwest wind resembling the Chinook or Foehn winds, which sometimes lasts for nearly a week; they are brisk, dry, hot, and more or less injurious to certain classes of vegetation. They are occasionally observed between the middle of June and the middle of August and appear to result from an area of low pressure central to the north of Texas, which causes the southeast winds to back to the southwest. The best defined wind of this character, and one which was the cause of general remark at the time, occurred from July 17 to 19, 1886, but such winds were not afterwards noticed to any extent until they were reported from June 18 to 22, 1891.

(*b*.) The Northerly Winds or Winter Monsoons.

On the monthly charts of prevailing winds, the north winds first appear distinctly in December. They then occupy the most of Texas east of the one-hundredth meridian, with the same indefiniteness, as before, as to the eastern limit.

On the January map their territory is not appreciably larger, while on the February map it is somewhat smaller. On the March map these winds do not appear at all.

From an examinination of the weather maps it appears that occasional north winds, lasting several days, may occur in all months except the summer ones. They are occasional during the spring and autumn, and prevalent, and often long-continued, during the winter.

They occur, for the most part, with anti-cyclones in latitudes north of Texas and east of the meridian of 110°. The control of the Texas winds by these centers in winter continues as the latter travel eastward until the latter approach or even reach the Atlantic coast. Whatever the position of the center of high pressure, within the limits above given, its effect in Texas, and sometimes far northward, is to give generally north winds.

There are also north winds of cyclonic origin, covering much more territory in the rear of cyclones in winter than in summer. An illustration can be found on the weather map for December 25th, a. m., 1890. In the case of the rare cyclones with a cold center, which is apparently an ordinary cyclone turned upside down, it is, of course, the advancing or eastern side which promotes the occurrence of north winds over the plains and in Texas. Such a case may be found on the morning weather map for February 25, 1891.

The winter north winds differ from the summer south ones in several respects other than those of direction and season. They are less persistent under adverse circumstances. They show less determination to push their way and are more easily suppressed, even in the midst of their proper season. They are most easily displaced on the coast, while the south winds are most persistent there. Indeed, they sometimes exist in the interior, while they are not present on the coast. For an illustration, see the weather maps for the morning and evening of November 28, 1891.

The north winds are usually clear or clearing. Their velocity is generally higher than that of the summer south

winds. They cause a fall in temperature, but this is often slight. Occasionally the cyclonic or anti-cyclonic conditions are especially favorable to the setting up of the north winds. When this happens, and especially when both combine to the same effect, the north winds may come down the plains with great velocity, with a sharply defined head of cloud like a battering ram, replacing warm and stagnant air and causing a sharp and great fall of temperature. These are the well-known Texas "northers." The "northers" are thus exaggerated cases of the prevailing north winds of winter on the southern part of the plains. They may also occur during the spring and autumn, and are then more noticeable to the residents because the chill they bring is more appreciable, and because a wind from the north is then less usual. When the conditions under which they are formed are especially favorable, they push their way far southward. They give tempestuous weather over the western part of the Gulf of Mexico. They even sometimes bring frost and snow on the high-land coffee plantations of Gautemala and Honduras, and, crossing the peninsula of Tehuantepec, they are sometimes encountered by ships far toward the equator on the Pacific ocean.

The conditions especially favorable to the formation of "northers" fall easily under two types. The first, or essentially cyclonic type, was recognized by Professor Ferrel. It is where a warm winter cyclone is closely followed by a mass of cold air. The pressure in the rear of the cyclone rises sharply, the heavy air behind, coming directly from the north, presses closely on it, and a sharp fall of temperature with northerly winds results. This is most common with winter cyclones originating in or crossing the western Gulf or southern Texas. The warm vapor laden air is suddenly displaced by the cold, dry, piercing air from the north, and the very contrasts between the two unlock energies which promote the latter. Good specimens of this type can be seen on the weather maps for the forenoon of December 3, 1890; also the afternoon of December 7 and morning of December 8;

also the afternoon map of January 1, 1891, and again for the forenoon of March 8, 1891.

The second type is anti-cyclonic. An anti-cyclone of especial intensity, or in an especially favorable position, sends a great stream of northerly winds down the plains, and these, replacing the milder air there, produce phenomena like the preceding, though generally not so marked. Illustrations can be found on the weather maps for November 18, 1890, and the forenoon of February 4, 1891.

In connection with the winter monsoons when cyclonic and anti-cyclonic conditions both favor the north winds, Dr. Cline has noted a character of cloud which is more common than any other. It has the appearance of several large rolls or sheets of black cotton batting each rolling over the other and extending from east to west above the prairie until they pass from view below the horizon. The appearance of these clouds is very striking.

It appears, therefore, that over Texas, and often far to the northward, there is a periodic seasonal wind, the air flowing northward in summer, southward in winter. In so far these winds have the ear-marks of monsoons proper, with complete reversal of directions. They have the necessary proximity of land and sea, the existence of great plains nearly bare of forest vegetation and gradually increasing in elevation above sea-level, and the necessary background of high mountains. To put the case beyond the possibility of doubt, it remains to be seen if these winds have the other ear-marks of the monsoons.

First, the continental area must have a higher temperature in summer and a lower in winter. That the latter must be true is so self-evident that it is not necessary to refer to the meteorological charts for its proof. The winter air over the semi-tropical Gulf must be decidedly warmer than that over the bare continental plains to the north. As to the summer, it is evident that the continental plains must be warmer in the daytime, cooler at night. The more ele-

vated plains become the warmer compared with air at the same level over the ocean. Late in the afternoon this difference reaches a maximum and may be very great, and a strong inland northerly current is in progress. This condition lasts well into the night and again begins the next morning immediately after sunrise. Whether the cooling to the minimum at sunrise is enough to greatly retard this northward movement, or even to bring it to rest and perhaps start a return current, is a matter for observation to decide. The decision appears to be in the negative, which is in accordance with the facts in other monsoon regions. Moreover, if a return even started during the night the phenomenon would become a simple land and sea breeze, and the theory of such a breeze is absurd when applied over a territory part of which is from five hundred to one thousand miles from the sea.

Charts representing isotherms reduced to sea-level (when this reduction is correctly made) may be safely used in the study of monsoonal winds, for the contrast of temperatures at any level, to which the motion is due, is the contrast between the temperature of the plain and that of air at the same sea level in adjacent lower regions. As the temperature decreases more rapidly in a vertical direction in free air than on the slopes of mountains, the charts of isotherms reduced to sea-level must show smaller contrasts of temperature than are actually existent for producing monsoons. Armed with this fact, Dr. Hann's excellent chart of July isotherms for the globe may be examined.* It is here found that the reduced temperature of Texas, Oklahoma, Colorado, and the western parts of Indian Territory and Kansas have about the same mean temperature as the western Gulf and Yucatan. As the elevation of the States is greater, a steady monsoon from the south in July should result, except in so far as it is interrupted by cyclonic and anti-cyclonic action. The same thing would in its degree be true for the neighboring months.

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*Hann (Julius). Juli-Isothermen. [*In* Berghaus (*dr.* H.) Physikalischer atlas, etc., fol. Gotha, J. Perthes, 1886. Map no. 29.]

The other test to apply is that of the differences in atmospheric pressure between the plains and Gulf in summer and winter. Here, again, it is safe to employ charts with the isobars reduced to sea-level. Turning to Dr. Hann's isobaric chart for January, it appears that the mean pressure increases from the Gulf northward, culminating in the Dakotas and Montana. The mean difference between the Gulf and this center in January is five millimeters, or a fifth of an inch—sufficient to give the air a tendency to move southward over the plains. In July the isobar of 760 millimeters runs nearly along the hundredth meridian from the foot of Yucatan to Manitoba, while there is lower pressure to the northward and southward, with, on the whole, a more rapid decrease to the northward. On the more detailed forenoon and afternoon normal-pressure charts for the months, now in manuscript in the Weather Bureau, increase in pressure from north to south in July is decidedly greater, being about a fifth of an inch. These charts are probably based on a larger number of synchronous observations taken with carefully compared instruments than are Dr. Hann's, being from fifteen years of the records of the Signal Service.

It appears, therefore, that, on the whole, the requirements for monsoons are present, but that the resulting monsoons are very much disturbed (sometimes promoted, sometimes suppressed) by the cyclones and anti-cyclones that are almost constantly crossing their area. There remains to be made the explanation of the fact that the summer monsoons last longer and are more persistent than the winter ones. This is the more difficult because all the conditions already mentioned are more favorable for the winter north winds than for the summer south winds. For this there seems to be no better way than to fall back on the mean wind direction prevailing in summer over the Gulf. The trade-winds in summer flow into the Gulf from the east and southeast, and reach the Texan coast nearly perpendicular to it. On the land, the surface winds are forced northward by the con-

tinental divide. Thus the gentle summer monsoons receive a push from behind, and this gives them more persistency and constancy than the more distinctly monsoonal and more violent winter winds from the north. Here again are reproduced the conditions of the monsoons of the Indian ocean, more especially those of the Arabian sea, where there exists a very similar aid from the general circulation.

To summarize, briefly, for this interesting set of monsoons: The air flows northward from the western part of the Gulf of Mexico from March to October, and in the summer reaches our northern boundary along a strip about ten degrees wide. It flows south during the winter in Texas, but is subject to sudden accessions called "northers," due to cyclonic or anti-cyclonic action. This action also frequently disturbs the summer south winds, and the latter are re-enforced by winds prevailing during their season in the Gulf.

These winds play a very important part in the climate of all Texas, except the extreme west. The southerly winds (or southeasterly toward the central or western part of the State) bring coolness and comfort with them in the very season coolness is most needed. The surgeon of the military station at Austin says: * "Although the sun is extremely hot in summer, the temperature of the atmosphere is much modified by the southeasterly breeze that blows almost continuously during the twenty-four hours." At Fort McKavett, which is northwest from San Antonio and about as far from that place as the latter is from the coast, the surgeon speaks of † "the delightful breezes of morning and evening throughout the latter part of the spring and the entire summer and fall." Fort McKavett is near the western margin of the summer monsoon. At Fort Richardson, near the Red river and south of southwestern Indian Territory, these winds are referred to as constant.

* U. S. War Department, Surgeon General's office. Report on the hygiene of the U. S. Army, etc., 8°, Washington, 1875, p. 183.

† *Ibid.*, page 217.

The north winds or winter monsoons are not so favorable for Texas, but at the same time they are not less favorable than are the prevailing westerly winds over the States far to the north of it. The " norther " is an occasional phenomenon, not more common than the "cold wave" of the upper Mississippi valley. Severe and destructive " northers " are quite exceptional.

The eastern limit of the Texan summer monsoons has already been referred to as not easy to define. There is no good evidence of them east of the Mississippi river south of the Ohio except on the Gulf coast. A prevailing south wind in June occurs at Cairo, but not below that place until the vicinity of the Gulf is reached. New Orleans has a northeast wind more than a quarter of the time in May, but no wind there reaches this frequency during the other months of the year. With Mobile the case is different. A south wind is the most frequent there from March to June, agreeing so far with the summer monsoon, but it fails with July, just when this monsoon is best established in Texas and northward. In September a prevailing north wind sets in, two months before it sets in on the Texan coast, and this wind continues until February. A south or southwest wind prevails at Pensacola from May to July and a north wind occurs in winter, reaching, however, a frequency of one-fourth only in November.

It appears that on the Gulf coast the monsoons are felt at Mobile and perhaps a little further eastward. The winter monsoon comes on prematurely at Mobile.

SUMMER HOT WINDS ON THE GREAT PLAINS.

BY

Isaac Monroe Cline.

[Read before the Society, January 20, 1894.]

During the past fifteen or twenty years that part of the United States which is between the 95th and 102d meridians has been settled rapidly by citizens engaged to a greater or less extent in agricultural pursuits. In following their avocations they have occasionally met with a peculiar climatic feature not previously noted in that section, namely, hot winds in summer, or currents of air with higher temperatures than that of the general winds.

On account of the striking peculiarities of these winds an opinion has gained prevalence that they are of frequent occurrence and extensive in their scope and damage, despite the fact that they have only occurred with sufficient intensity to attract the notice of observers in ten years out of the past twenty-three and have not been of sufficient extent and severity to damage crops to a degree worthy of notice except in three or four years. Even when severe their destructiveness is confined to narrow limits, and, except in rare cases, no great damage results from their effects except to a field here and there, and these are only damaged in spots. Aside from the scientific value of a collation and study of the records relating to these winds, the false impressions which prevail relative to their extent and damage have led to a strong desire by those interested in the development of that section for the publication of facts connected therewith, so that the public may know the character and extent of such winds, and when and under what conditions they may

be expected. In 1890 Senator P. B. Plumb, of Kansas, addressed a note to the Signal Office asking that a paper be prepared and published on this subject; nothing, however, was published in this connection. Almost all that has been published on this subject is contained in two papers, one on "The hot winds of the Plains,"* by Mr. G. E. Curtis, and the other on "Hot winds in Texas May 29 and 30, 1892,"† by the writer.

My attention was first attracted to this subject in 1886, while stationed as observer at Abilene, Texas. A personal knowledge of these winds and their effects in connection with agricultural interests led me to collect all the facts recorded in connection with these phenomena since 1870. The records of voluntary observers and reports of agricultural bodies, together with those of State weather services, have been examined in this connection, and all notes bearing on the subject have been extracted and will be found in the succeeding pages of this paper.

In the following transcript of records the more important meteorological phenomena in connection with each period of hot winds are noted. Three charts, showing the three types of pressure, with distribution of temperature, state of weather, and direction of wind at the time of occurrence of hot winds in summer over different parts of the eastern Rocky Mountain slope, are printed herewith (plates 4, 5, and 6). These plates are copied from the United States weather map, except that the data have been entered and isobars drawn for the western portion of the country where not already drawn, and the isotherms have been drawn for each five degrees. In a few instances the isobars on the original charts have been redrawn on these charts in the light of fuller knowledge. On each chart is shown the movement of the area of low pressure with which the hot winds oc-

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**In* Seventh Biennial Report of the Kansas State Board of Agriculture. 8°. Topeka, 1891, part 2, pp. 162–183.

†*In* American Meteorological Journal. 8°. Boston, Ginn & co., 1893, Feb., vol. 9, no. 10, pp. 437–443.

curred from the date of its appearance until it moved away to the eastward. In locating the position of the low pressure on different dates over the eastern slope where reports were scattering I have been governed by wind directions and the fall in pressure and have thus approximated its position. Over three hundred weather charts have been consulted in connection with this work.

Hot Winds, and General Meteorological Conditions Prevailing at Date of Their Occurrence.

1874, *July* 24, *Red Cloud, Nebraska.*—"1 p. m., hot winds from the southwest; 5 p. m., strong indications of rain; 9 p. m., zigzag lightning, heavy thunder in south." J. H. Kellogg, voluntary observer.

These hot winds accompanied an area of low pressure which appeared over Montana July 21 and moved slowly southeast to the central portion of the eastern Rocky Mountain slope by the 23d, where it remained nearly stationary until the morning of the 25th, after which it moved rapidly eastward. During this time the pressure was 30 inches, or slightly above, along the coast of Oregon and Washington. Partly cloudy to cloudy weather prevailed from the Pacific coast to the divide, and the general tendency of the atmosphere appeared to be eastward across the divide toward the low pressure area. An area of high pressure covered the southeastern portion of the country.

1874, *August* 9 *and* 10, *Red Cloud, Nebraska.*—"Hot winds from the south; smoky atmosphere." J. H. Kellogg, voluntary observer.

This period of winds accompanied an area of low pressure which appeared over northern Montana on August 6, following an area of low pressure which was moving eastward from the upper Missouri valley.

The low pressure area moved from northern Montana southeast along the eastern Rocky Mountain slope, reaching northern Nebraska on the morning of the 9th. It moved slowly southward on the 9th, northward on the 10th, and

appeared central over northern Iowa on the morning of the 11th, after which it moved rapidly eastward. During this time the pressure was above 30 inches over the north Pacific coast, with partly cloudy to cloudy weather and an apparent general tendency of the atmosphere to cross the divide in an easterly direction toward the low pressure area. The pressure was above 30 inches over the southeastern portion of the country.

1881, *June* 10 *to* 20, *Wellington, Kansas.*—"The most noteworthy features of the climate during this month, as recorded by the tri-daily observations, were its periods of hot winds, noted in the second ten days of the month, blowing from the southwest.

"The rainfall for the entire month, as recorded, was 3.18 inches on five days.

"The direction of the upper currents of air were noted blowing from the west constantly from the 12th to the 30th; from the southwest from the 4th to the 12th, and from the northwest the first three days of the month." John H. Wolfe, voluntary observer.

This period of hot winds accompanied an area of low pressure which appeared over Montana on June 6, and moved southeast along the eastern Rocky Mountain slope, reaching northwestern Kansas on the 11th, after which it moved rapidly to the northeast and disappeared over the lakes on the 13th. During this time the pressure along the Pacific coast ranged about 30 inches. The general tendency of the atmosphere was ill defined, but apparently across the divide in an easterly direction toward the low pressure area, and the weather to the west of the divide was partly cloudy to cloudy.

1881, *July* 19 *and* 20, *Wellington, Kansas.*—"Hot winds four hours on the 19th and twenty-four hours on the 20th.

"Rainfall for the month measured 3.21 inches. Rain fell on six days.

"The direction of the upper strata of air was constantly from the west throughout the entire month." J. H. Wolfe, voluntary observer.

These winds accompanied an area of low pressure, which made its appearance over Montana on the morning of July 17 and moved southeast, reaching northwestern Nebraska

on the morning of the 19th, after which it moved slowly eastward. During the development and progress of this low pressure area an area of high pressure was moving in over Oregon and Washington from the Pacific ocean. The general direction of the atmosphere over the Pacific slope was not very well defined, but was apparently across the divide toward the low pressure area, and partly cloudy to cloudy weather prevailed to the west of the divide.

1882, *September* 12 *and* 13, *Wellington, Kansas.*—"The most notable feature of the month was the hot, sweeping winds noted on the 12th and 13th, this being an unusual occurrence for a September month. Rainfall, 3.87 inches. Rain fell on six days.

"The upper strata (cirrus clouds) was observed from the west on eighteen days. On the 12th analogous hot-air currents were observed blowing from due southwest, continuing about four hours during the middle of the day. On the 13th for two hours similar hot winds were noted blowing in puffs alternately with cooler air." J. H. Wolfe, voluntary observer.

1882, *September* 12 *to* 14, *Leavenworth, Kansas.*—"A very hot and extremely dry wind set in from the southwest at 1 p. m. on the 12th. At 4 p. m. the maximum temperature, 101°, occurred, being the highest recorded during September since the establishment of the station. The relative humidity fell to 17 per cent. This hot wind continued through the remainder of the day and the 13th and 14th. Vegetation was withered and burned up." Observer, Signal Service.

1882, *September* 12 *to* 15, *Lawrence, Kansas.*—"The month (September) was chiefly noted for the hot blast of the 12th, which was repeated with somewhat diminished intensity on the 13th, 14th, and 15th. During these simoons the air was excessively dry, the relative humidity sinking to 7 per cent. on the afternoon of the 12th. The fierce, dry heat burned the foliage of the trees, so that they crumbled to powder at the touch of the hand. The nights following these withering days were comparatively cool, the mercury sinking from 105° on the 12th to 65° on the morning of the 13th, and from 101.5° on the 13th to 66° on the morning of the 14th." Prof. F. H. Snow, University of Kansas.

1882, *September* 12, *Fort Riley, Kansas.*—"Very hot wind. At noon the thermometer stood at 103° and rose to 108° in less than an hour and remained at that temperature for about three hours."

1882, *September* 12, *Clay Center, Kansas.*—"Hot southwest winds."

1882, *September* 12, *Clinton, Missouri.*—"Hot south wind."

This period of hot winds accompanied an area of low pressure which made its appearance over central Montana

on the morning of September 8, moved slowly eastward, developing into a V shaped depression, which extended south on the 11th to Kansas. From the 12th to the 13th this depression remained stationary and then moved off slowly to the eastward. During this time the pressure was about 30 inches over the western portions of Washington and Oregon, with cloudiness and rain and a fairly well defined tendency of the atmosphere to cross the divide toward the area of low pressure.

During the development of the low pressure area over the eastern Rocky Mountain slope an intense cyclone moved from the central portion of the Gulf of Mexico northward to Nova Scotia. (See plate 4.)

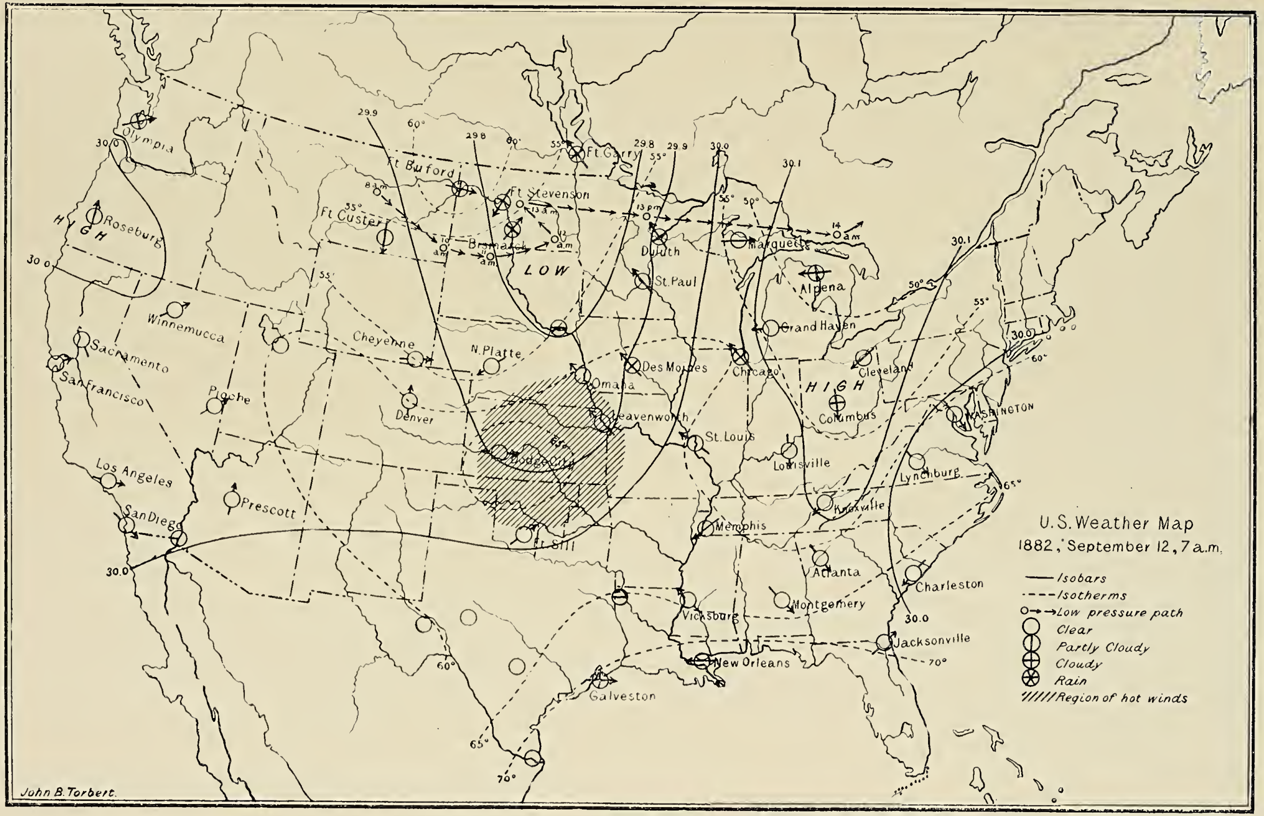
1883, *July* 19 *to* 22, *Manhattan, Kansas.*—"Very warm and dry southwest winds proved very injurious to crops."*

These winds accompanied an area of low pressure which was apparently developing over Wyoming on July 18 and was over the central portion of the eastern slope, with its greatest depth in Kansas and Nebraska from the morning of the 19th until the morning of the 23d, after which it moved rapidly eastward and reached the lower lakes on the morning of the 24th. During the development and progress of this low pressure area an area of high pressure was off the coast of Oregon, with pressure above 30.20 inches. With few exceptions, reports from the Pacific slope were missing and very little information was at hand from which to draw conclusions as to the general direction in which the atmosphere was moving, but conditions, it appeared, were such as would force it from the Pacific slope across the divide toward the low pressure area.

1886, *June* 15 *to* 17, *Mason, Texas.*—"Hot southwest winds." I. M. Cline.

An area of low pressure extended over the plateau region from Fort Garry to Yuma on the 10th and 11th, deepened over the central Rocky Mountain slope on June 12th, and

*G. E. Curtis, op. cit., p. 165.



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remained over that section until the 14th. It then moved slowly southward and reached northern Texas on the morning of the 16th, after which it moved rapidly eastward. Rain fell over the central and southeastern slopes previous to the 16th, but the latter date was cloudless over the southeastern slope. During the development and progress of this low pressure area the barometer was generally above 30 inches along the Pacific coast, and the general tendency of the atmosphere was from the Pacific, across the divide, toward the low pressure area, with cloudiness and some rain. A general depression prevailed from the lakes to northwestern Texas during the latter part of this period, and the barometer was rising over the plateau region.

1886, *July* 4 *to* 7, *Hamilton County, Nebraska.*—"Hot winds." John Ellis, voluntary observer.

These winds accompanied an area of low pressure which appeared over the central portion of the eastern slope on the morning of July 2 and moved northward to the northern slope and upper Missouri valley on the 4th and 5th, forming a **V** shaped depression, with its apex over the southeastern slope. On the 6th the low pressure moved rapidly eastward, and an area of high pressure moved in over the northern slope. During the development and progress of this area of low pressure the barometer was generally about 30 inches over the western portions of Oregon and Washington, with cloudy weather and rain and a well-defined tendency of the atmosphere to cross the divide toward the area of low pressure. The weather was generally cloudless over the central and southeastern slopes.

1886, *July* 13 *to* 18, *Hamilton County, Nebraska.*—"July 13; hot winds." J. Ellis, voluntary observer.

1886, *July* 13 *to* 18, *Atchison, Kansas.*—"July 13; hot winds from the northwest began about 10.00 a. m. and continued until in the evening." Dr. H. B. Horn, voluntary observer.

1886, *July* 13 *to* 18, *Independence, Kansas.*—"July 14; hot winds from the north." J. M. Altafer, voluntary observer.

1886, *July* 13 *to* 18, *Wellington, Kansas.*—"On the 15th (July) hot waves of wind with temperature 104°; wind moving from southwest at 12 miles per hour; crops and vegetation injured to some extent along the southwest edges of fields; blades of corn-stalks turned entirely white and looked as if scalded, then breaking in crisps. Total rainfall for month was 5.65 inches, which is 1.43 inches above the average July mean for the past eight years." J. H. Wolfe, voluntary observer.

1886, *July* 13 *to* 18, *Northwest Texas.*—"On the 18th (July) hot southwest winds in many counties; were very dry and parching." I. M. Cline.

This period of hot winds accompanied a very peculiar distribution of pressure, and one rarely met with. Low pressure prevailed along the eastern slope from July 9 to 18, inclusive; its greatest depth was over Montana and the Dakotas until the 13th, when a well defined low pressure area developed over the upper lakes and upper Mississippi valley; the general depression, however, continued along the eastern slope. On the morning of the 14th a depression appeared over western Nebraska, and this had developed into a well defined low pressure area on the morning of the 15th, which moved southward and extended over the southeastern slope from the 16th to the 18th, disappearing on the 19th. During the prevalence of this depression the pressure was above 30 inches along the coast of Oregon and Washington, with a well defined tendency of the atmosphere to cross the divide toward the area of low pressure, and this was accompanied by some cloudiness and rain. Generally cloudless weather prevailed over the central and southeastern slopes on the 13th, 14th, and 15th, and the southeastern slope on the 16th, 17th, and 18th.

1886, *August* 12 *to* 16, *Atchison, Kansas.*—"Hot winds blew from the south on the 12th, 13th, 15th, and 16th." Dr. H. B. Horn, voluntary observer.

1886, *August* 12 *to* 16, *Wellington, Kansas.*—"Hot winds noted for two hours' duration on the 16th (August), 2 p. m. to 4 p. m. Rainfall for the month, 2.96 inches, is .15 inch below the average for the past eight years; rain on seven days." John H. Wolfe, voluntary observer.

This period of hot winds accompanied an area of low pressure, which appeared over eastern Colorado on the morn-

ing of August 9, moved northward and eastward to South Dakota by the 13th, and then moved back southward into western Kansas during the 14th and 15th, after which it moved slowly to the eastward. During this time the barometer stood near 30 inches over western portions of Oregon and Washington, with some cloudiness and rain. While the general movement of the atmosphere was not very well defined, its tendency was eastward across the divide toward the low pressure area. Cloudless weather prevailed during this time over the central and southeastern slopes.

1887, *July, Wellington, Kansas.*—"The effects of the temperature during this month (July), in connection with hot winds which occurred on the 13th, 17th, and 27th, 28th, and 29th, with an average duration each day of two and one-half hours, caused to some extent in some localities on upland farms entire destruction of the corn crop; precipitation, 3.45 inches, which is .69 of an inch below the nine years' average." John H. Wolfe, voluntary observer.

1887, *July, Independence, Kansas.*—"We never saw corn wilt so rapidly as during the hot waves of the 16th and the 25th to the close of the month. The rainfall of 3.21 inches is only .93 of an inch below the July average for fourteen years." J. M. Altafer, voluntary observer.

1887, *July, Bellville, Kansas.*—"On the 29th, from 1 p. m. till 5 p. m., the thermometer stood at 106°, with a hot southwest wind blowing, damaging crops to a considerable extent." A. B. Graves, voluntary observer.

1887, *July, Brownsville, Nebraska.*—"On the 29th the temperature reached 106°, with brisk south wind, hot as the breath of a furnace. Corn and all vegetation are about burned up; grass dead. Corn blades are dead from three feet upward on the stalks and the shuck appears frost-bitten. In the main, corn on the low land is yet good. Apples are baked on the trees in some instances. On the 29th, 6 p. m., a thin film of cloud appeared in the northwest and moving in a northwesterly direction." George D. Carrington, voluntary observer.

1887, *July, Topeka, Kansas.*—Hot winds last of July.

1887, *July, Mercer County, Missouri.*—Hot southwest winds July 29th.

The period of hot winds from the 13th to the 16th accompanied an area of low pressure which first appeared over eastern Montana on the morning of July 13, moved eastward and southward, and was central over western Nebraska on the 15th. On the 16th it moved northeast to

western Minnesota and disappeared to the eastward on the 17th. The pressure during this time was above 30 inches along the coast of Oregon and Washington, and the atmospheric movement was generally very well defined from the Pacific coast across the divide toward the area of low pressure, with considerable cloudiness but no rain from weather-bureau stations. Cloudless weather prevailed over the central slope.

The period of hot winds from July 25 to 29 accompanied an area of low pressure which appeared over the central plateau region on the 24th, moved eastward to the central slope on that date, and on the 25th and 26th developed into a trough of low pressure extending from northern Minnesota to New Mexico. On the 27th the low pressure backed to western Colorado, and the barometer rose about .20 of an inch over the central and northern slopes, but on the morning of the 28th the low pressure had moved back to the central slope and on the 29th extended from eastern Colorado to northern Minnesota, after which it moved rapidly eastward to the north of the lakes. During the progress of this low pressure area the barometer stood above 30 inches along the coast of Oregon and Washington, with more or less cloudy weather and rain and a fairly well defined movement of the atmosphere from the Pacific eastward across the divide toward the low pressure area.

1887, *August* 4, 10, *and* 14, *Wellington, Kansas.*—"Hot winds for several hours each day on the 4th, 10th, and 14th were noticed, but no material damage resulted; rainfall, 5.07 inches, which is 2.41 inches above the nine years' average, this being the largest rainfall for August on our record and was well distributed during the month." J. H. Wolfe, voluntary observer.

The hot winds of August 4 accompanied an area of low pressure which extended along the eastern slope on the morning of August 1, and did not change, other than to deepen, until the 3d, when it commenced moving eastward. An area of high pressure, which was off the coast of Oregon

on the 1st, moved steadily eastward, and on the 5th occupied the central slope, while the low pressure area moved eastward to the lower lakes and central valleys. Cloudy weather prevailed in advance of the high pressure area as it moved eastward. Cloudless weather prevailed over the central and southeastern slopes.

The hot winds of August 10 accompanied an area of low pressure which developed over eastern Colorado on the morning of August 8 as a secondary to a low pressure which moved eastward from Montana on the 7th. This latter low pressure area soon disappeared, while the one which had developed over the central slope increased in energy and moved slowly northeast until the 10th, after which it moved rapidly eastward and disappeared to the north of the lakes on the 11th. The barometer during this time continued above 30 inches along the coast of Oregon and Washington, with cloudiness to the west of the divide, but no well defined atmospheric circulation. Cloudless weather prevailed on the 9th and 10th over the central and southeastern slopes.

The hot winds on August 14 were preceded and accompanied by the following conditions: On the 12th the barometer was above 30 inches over the central slope, with cloudy weather and rain. A low pressure area was advancing from the northern plateau region, and this displaced the high pressure area over the central slope on the morning of the 13th, with its greatest depth over Nebraska. On the 14th the low pressure extended from Lake Michigan to Colorado, with its greatest depth over southeastern Iowa, but a secondary disturbance was developing over western Kansas, with clearing weather. On the 15th the low pressure moved rapidly eastward. During this time the pressure was about 30 inches over the western portions of Oregon and Washington, with a very well defined movement of the atmosphere from the Pacific slope across the divide toward the low pressure area, with more or less cloudiness.

1888, *July* 13, *Gibson, Kansas.*—"Hot winds from the southwest on the 13th." Chas. M. Bell, voluntary observer.

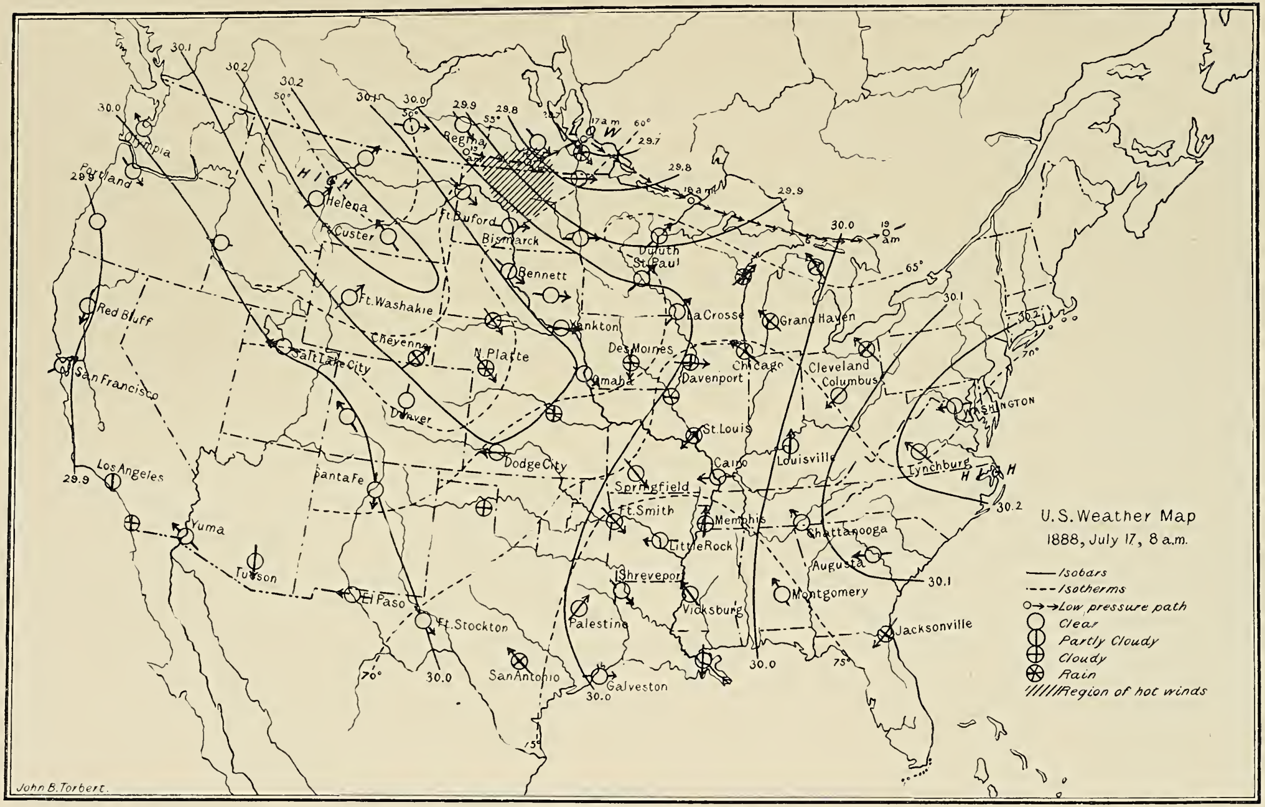
This period of hot winds accompanied a low pressure area which appeared to the north of Montana on the morning of July 10 and extended southward to northern Texas by the morning of the 13th, with its greatest depth over northern Nebraska. After the 13th the low pressure moved off to the southeast and filled up. During the development and progress of this low pressure area the barometer was above 30 inches along the Pacific coast, with cloudy weather and rain, and there was a well defined movement of the atmosphere from the Pacific slope across the divide toward the area of low pressure. The weather was cloudless over the central and southeastern slopes.

1888, *July* 17 *to* 19, *Parkston, North Dakota.*—"Hot southwest winds July 17 to 19." J. J. Swartzy, voluntary observer.

This period of hot winds accompanied an area of low pressure which appeared in the vicinity of Qu'Appelle on the morning of the 15th and moved slowly eastward to the north of Minnesota on the morning of the 17th and passed eastward to the north of the lakes on the 19th. During the progress of this low pressure eastward an area of high pressure moved southeastward from Oregon and Washington; its crest reached Colorado and Wyoming on the 18th and covered the entire eastern slope on the 19th. Cloudless weather prevailed over the Dakotas and eastern Montana, while to the north, east, and south generally cloudy weather prevailed, with more or less rain. (See plate 5.)

1888, *July* 24 *to August* 2, *Independence, Kansas.*—"July—Hot winds or waves of hot air were felt during the middle of the day toward the close of the month, but we have had no siroccos, and corn is fresh and green except at the lower blades and tassels; but 1.04 inches of rain since the 22d of June." J. M. Altafer, voluntary observer.

1888, *July, Tecumseh, Kansas.*—"Hot winds, southwest, on the 24th, 25th, and 26th began to tell injuriously on crops." W. L. Dunlap, voluntary observer.



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1888, *July, Salina, Kansas.*—"The last ten days have been intensely hot and the scorching furnace winds since the 26th have nearly ruined the corn crop. No longer period of such winds has occurred since August, 1874. The hot winds of July 26 continuing to August 3 completely ruined the corn crop; fields which promised sixty bushels per acre were changed to dry fodder during that period." John H. Gibson, voluntary observer.

1888, *July.*—"Hot winds during the last part of July." State agent, Kansas.

The hot winds of July 24 to 30 were really two distinct periods with the same low pressure area. The low pressure appeared over Montana on the morning of the 24th, moved southeast, and covered the central slope on the 26th and 27th, after which it moved westward into Utah, and the barometer rose .20 of an inch on the 28th along the eastern slope. On the morning of the 29th the low pressure area had moved eastward and covered the central and northern slopes, where it remained stationary until the morning of the 30th, after which it moved rapidly to the eastward. During the prevalence of this low pressure over the eastern slope the barometer was above 30 inches over the western portions of Oregon and Washington and there was a well defined movement of the atmosphere from the Pacific slope across the divide toward the low pressure area. On the 29th and 30th the high pressure area moved rapidly eastward from the Pacific coast and an area of low pressure made its appearance over Oregon on the 31st. Cloudless weather prevailed over the greater portion of the eastern slope from the 24th to the 30th.

The hot winds during August the 1st and 2d, while apparently a continuation of the preceding periods, were quite independent and accompanied the low pressure area which was over eastern Oregon on the 31st of July, and which moved rapidly eastward, reaching the eastern slope on the afternoon of the 1st, where it remained nearly stationary for 24 hours and increased in intensity. It then moved rapidly to the northeast, but low pressure continued over the central slope until the morning of the 4th, when the barometer rose to about 30 inches. As the low pressure

moved eastward from the Pacific coast the barometer rose to over 30 inches and remained so during this period. Cloudless weather prevailed over the central and southeastern slopes on the 1st, 2d, and 3d.

1889, *June* 18, *Lincoln County, Nebraska.*—Wheat in dough damaged by hot winds June 18th.*

These hot winds accompanied an area of low pressure which appeared to the north of Montana on the morning of June 15 and moved slowly southeastward and extended down the eastern slope until the 18th, after which it moved rapidly to the northeast, reaching the upper lakes by the morning of the 19th. During the development and progress of this low pressure area there was an area of high pressure in the Pacific off the coast of Oregon. The movement of the atmosphere was generally eastward across the divide and cloudy weather, with some rain, prevailed over the Pacific slope. Cloudless weather prevailed on the 18th over Nebraska, Kansas, and the Indian Territory, while to the north, east, and south it was generally cloudy.

1889, *July* 4 *to* 7.—Hot winds were reported from several counties in Nebraska and in Kansas on these dates and materially damaged the small grain crop where they occurred.*

This period of hot winds accompanied an area of low pressure which appeared in the vicinity of Calgary, Northwest Territory, on the morning of July 3, and moved eastward and southward along the eastern slope, reaching the central portion on the morning of the 5th and remained there until the night of the 7th, when it filled up. During the progress of this low pressure area southward the barometer was about 30 inches over the western portions of Oregon and Washington, and the movement of the atmosphere was generally across the divide toward the low pressure area, with partly cloudy to cloudy weather. Cloudless weather prevailed over the eastern slope from Nebraska to Texas.

* G. E. Curtis, op. cit., pp. 169–172.

1889, July 14 to 19, *Gibson, Kansas.*—"Hot winds from the southwest on the 16th and 17th, the heat coming in puffs and rolls." Chas. M. Bell, voluntary observer.

1889, *July* 14 *to* 19, *Indian Territory, Fred, Chickasaw Nation.*—"Hot winds; direction, southwest; temperature, 106°; very parching and drying to all grass and vegetation, especially melon vines and the tassel or top of growing corn, while just under the ground, about one inch, the soil is plenty moist for growing vegetation, and I have seen corn crops ruined in ten days. When the corn ear is just forming the wind would come and dry up the pollen, and there would be no corn on the stalks, while the blades and stalks were still green until fall." W. S. Cook.*

1889, *July* 14 *to* 19, *Kansas, Lane County.*—"Hot winds on 16th and 17th from 10 a. m. to 5.30 p. m. each day—direction, southwest—drying up all vegetation."*

1889, *July* 14 *to* 19, *Kansas, Comanche County.*—"Hot southwest winds on the 16th and 17th, ceasing about 5 p. m. each day. Nearly all vegetation considerably distressed. Corn beginning to tassel suffered most."*

1889, *July* 14 *to* 19, *Kansas, Stafford County.*—"Hot winds on 17th—9 a. m. to 5 p. m; direction, southwest."*

1889, *July* 14 *to* 19, *Kansas, Grant County.*—"Hot winds from morning until evening on the 16th, 17th, and 18th; direction, southwest."*

1889, *July* 14 *to* 19, *Kansas, Trego County.*—"Hot winds, 10 a. m. to 5 p. m., on the 16th and 17th; direction, southwest. Corn badly withered. We think that the hot winds are generated from the soil by the sun's rays, as the waves of heat strike the face from the earth similar to opening the oven door of a hot stove."*

This period of hot winds accompanied an area of low pressure which appeared over northern Montana on July 13 and 14 and moved southeastward, covering the eastern slope on the 16th and 17th, with its greatest depth over Nebraska, after which it moved rapidly to the northeast, reaching Lake Michigan on the morning of the 18th. During the movement of the low pressure along the eastern slope the barometer was about 30 inches along the coast of Oregon and Washington, with generally cloudy weather, and the general tendency of the atmosphere appeared to be eastward across the divide toward the area of low pressure. Cloudless weather prevailed over the central slope on the 16th and 17th, while it was cloudy and raining to the east and north.

* G. E. Curtis, op. cit., pp. 169-172.

1889, *July, Gibson, Kansas.*—"Hot winds on the 26th and 27th (on the 27th they came from the north), the heat coming in puffs and rolls." Chas. M. Bell, voluntary observer.

1889, *July, Tribune, Kansas.*—"The hot winds of the 22d, 26th, and 27th burned the corn." S. B. Jackson, voluntary observer.

1889, *July, Shocky, Kansas.*—"The hottest wind of the season prevailed from 9 a. m. to late in the evening of the 27th. The peculiarity of this wind was that it was from the north, and was so hot that it heated all metals and solid substances so as to make them uncomfortably hot to the touch." T. R. Hornady, voluntary observer.

1889, *July, Kansas, Morton County.*—"Hot winds from the north and northeast were reported as prevailing each day from 10 a. m. to 4 p. m. Vegetation wilted, but not so badly as in the case of hot winds from the south. The hot winds were not steady, but came in frequently recurring waves; do not remember to have ever experienced hot winds from the north, and certainly not so notably warm as on this occasion."*

1889, *July, Kansas, Greeley County.*—"Hot wind from the north each day from 10 a. m. until 5 p. m."*

1889, *July, Indian Territory, Fort Supply*—"Hot winds on the 26th from 8 a. m. until 6 p. m. Direction south until 4 p. m., then changed to north. Vegetation already about all dried up, so not much affected. Maximum temperature, 111°; temperature reached maximum about 2 p. m. and remained above 100° until 5 p. m."*

1889, *July, Indian Territory, Fort Sill.*—"Hot winds from 10 a. m. until 9 p. m. of the 27th—direction south. The south wind blew with increasing force and rising temperature. Leaves of plants dropped. At 4 p. m. (sun time) calmed down suddenly and a strong north wind set in with thunder and lightning. Temperature fell in twenty minutes from 104° to 84°. Force of wind 6 to 8 on a scale of 10, and duration two hours. North wind wore gradually to west, with increase of temperature to 96°. At 6:30 p. m. wind turned to southwest, decreasing in force; temperature at 9 p. m., 90°. Wind suffocating to man and beast."*

1889, *July, Indian Territory, Anadarka.*—"Hot winds from 9:45 to 10:05 p. m. (railroad time) on the 27th. Direction, northwest; temperature, 106."*

1889, *July, Northwestern Texas.*—"Several counties in the panhandle suffered from hot winds on the 26th, 27th, and 28th." I. M. Cline.

This period of hot winds accompanied an area of low pressure which appeared to the north of Montana on the morning of the 23d, moved eastward and extended southward over the eastern slope, covering that territory during the 25th, 26th, and 27th, after which it moved rapidly to

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*G. E. Curtis, op. cit., p. 172.

the northeast. During the development and progress of this low pressure area the barometer stood at about 30 inches or above over the western portions of Oregon and Washington, and the general trend of the atmosphere was from the Pacific eastward across the divide toward the low pressure area, and this was very marked after the 24th. Cloudless weather prevailed along the eastern slope on the 26th and 27th.

1889, *August* 6 *to* 8, *Kansas.*—Hot winds from the southeast to the southwest were reported on these dates from Wallace, Morton, Greeley, and Trego counties. No special features are noted except that the wind was strong and vegetation became withered and dry and corn was damaged in some localities.

These winds accompanied an area of low pressure which appeared to the north of Montana on August 3, moved slowly eastward, and extended southward over the eastern slope until the 5th, when it divided into two distinct low-pressure areas, one to the north of Montana and the other over southeastern Montana. That to the north of Montana remained stationary, while that over the southeastern portion moved slowly southeast to northern Nebraska by the afternoon of the 7th, and both low areas moved rapidly to the lakes on the 8th.

During the development and progress of these areas of low pressure the barometer stood about 30 inches along the coast of Oregon and Washington, and the general tendency of the atmosphere was well defined from the Pacific coast across the divide toward the low pressure area. Partly cloudy to cloudy weather prevailed to the west of the divide. Generally cloudless weather prevailed during the 5th, 6th, and 7th over the central and southeastern slopes.

1889, *August* 19.—Hot south and southwest winds were reported from Decatur and Morton counties, Kansas, and Holt county, Nebraska, on August 19th, but no special features were reported.

These winds accompanied an area of low pressure which made its appearance in the vicinity of Calgary on August

14 and moved slowly southeastward and covered the central and northern slopes on the 19th, with its greatest depth over South Dakota; it moved rapidly eastward to the north of the lakes on the 20th. From the development of the low pressure area to its disappearance the barometer stood 30 inches or above over Oregon and Washington, with general, cloudiness and heavy rain, and the general movement of the atmosphere was well defined from the Pacific across the divide toward the low pressure area. Generally cloudless weather prevailed over Kansas and Nebraska on the 19th.

1889, *August* 25 *to* 29, *Fremont, Kansas.*—"Dry hot south winds prevailed from the 26th to the end of the month. All corn not matured is now dried up; also grass." E. M. Atkin, voluntary observer.

1889, *August* 25 *to* 29, *Kansas, Morton County.*—Hot winds 24th to 28th, inclusive, from 9 a. m. to 5 p. m.; direction, southwest. The effect on vegetation was very withering, sapping the moisture. The force of the wind was much greater than of any yet reported or than any before experienced that I can recollect; but the heat was not as intense or as constant as usual in hot winds.*

1889, *August* 25 *to* 29, *Kansas, Decatur County.*—Hot gusts on the 27th, continued on the 28th; dried up corn and cane; direction mostly south.*

1889, *August* 25 *to* 29, *Kansas, Greeley County.*—Hot winds from the south, 25th to 29th, inclusive.*

1889, *August* 25 *to* 29, *Nebraska, Furnas County.*—Hot winds 24th to 31st, also in Holt county from 19th to 31st.*

These hot winds occurred with an area of low pressure which made its appearance to the north of Montana on August 22, moved eastward to northern Minnesota on the 23d and 24th, and at the same time extended southward to the central slope. The low pressure area backed to northern Montana on the 25th with low pressure over the northern slope, after which it moved slowly eastward, and at the same time extended southward, and covered the central and northern slopes and the upper Missouri valley on the 28th, after which it moved rapidly to the northeast and disappeared to the north of the lakes on the 29th. During the prevalence of this low pressure along the eastern slope the

* G. E. Curtis, op. cit., p. 177.

barometer was 30 inches or above off the coast of Oregon, and the general tendency of the atmosphere was very well defined from the Pacific coast across the divide toward the low pressure area, with partly cloudy to cloudy weather. Cloudless weather prevailed over the central slope from the 24th to the 28th, inclusive.

1890, *June, Downs, Kansas.*—"On the 26th, hot winds, which burned the blades of corn; turned them white." J. B. Handy, voluntary observer.

1890, *June, Alton, Kansas.*—"On the 28th, at 1 p. m., thermometer registered 103°, with south wind. We have fortunately been free from hot winds, and crops are looking fine." A. H. Goddard, voluntary observer.

1890, *June, Gibson, Kansas.*—Hot winds during the latter part of the month.

These winds accompanied an area of low pressure which appeared over the northern slope and northern plateau on June 18, with pressure below 29.70 inches; the low pressure centralized over the central slope on the 22d and oscillated back and forth from South Dakota to Kansas from the 22d to the 27th, after which it moved rapidly to the eastward. During the development and progress of this low pressure area there was an area of high pressure in the Pacific off the coast of Oregon. Cloudy weather prevailed from the Pacific coast to the divide; cloudless weather prevailed over the central and southeastern slopes during the time these hot winds were reported.

1890, *July, Alton, Kansas.*—"Hot winds on the 8th wilted crops." A. H. Goddard, voluntary observer.

1890, *July, Manhattan, Kansas.*—"6th to 14th, hot winds almost every day." Prof. C. M. Breese.

These winds accompanied an area of low pressure which appeared to the northwest of Montana on July 3, moved southeastward along the eastern slope until the 6th, when it developed into two well defined low pressure areas, one over South Dakota and the other to the north of Montana, both of

which moved eastward on the 7th. Similar depressions also prevailed over the eastern slope from the 9th to the 14th. The pressure was about 30 inches along the Pacific coast, with cloudy and showery weather during this time, and the eastward tendency of the atmosphere from the Pacific across the divide toward the low pressure area was well defined. Generally cloudless weather prevailed from Texas to Nebraska at the time hot winds were reported.

1890, *July, Alton, Kansas.*—"Hot winds occurred on the 26th, 27th, 28th, 29th, and 30th; crops all burned up in this locality." A. H. Goddard, voluntary observer.

1890, *July, Manhattan, Kansas.*—"29th, hot winds from the south." Prof. C. M. Breese.

1890, *July, Downs, Kansas.*—"It has been so hot this month that there will be no corn harvested in this county this fall." J. B. Handy, voluntary observer.

This period of hot winds accompanied an area of low pressure which appeared to the northwest of Montana on the afternoon of July 23 and moved slowly eastward, reaching North Dakota on the 26th and extending southward over the central slope, where it remained nearly stationary until the 30th, after which it moved off rapidly to the northeast. During this time an area of high pressure was central in the Pacific off the coast of Oregon and Washington, and the general tendency of the atmosphere was from the Pacific across the divide toward the low pressure area. The weather to the west of the divide was generally cloudy, with some rain. Cloudless weather prevailed during the greater part of the time over the central and southeastern slopes.

1890, *August* 1 *to* 2, *Alton, Kansas.*—"Hot winds on August 1 continued all night." A. H. Goddard, voluntary observer.

1890, *August* 1 *to* 2, *Cunningham, Kansas.*—"August 1 and 2, very hot and dry, with hot winds." E. Shaw, voluntary observer.

1890, *August* 1 *to* 2, *Manhattan, Kansas.*—"August 1 and 2, hot winds." C. M. Breese.

This period of hot winds accompanied an area of low pressure which appeared to the north of Washington on

July 30, and on the 31st formed a **V** shaped area of low pressure extending from northern Nevada to Northwest Territory. On the morning of August 1 this moved out over the eastern slope, the central and northern portions of which were covered by this low pressure area during the 1st and 2d. Afterwards it moved eastward and developed on the 3d into a trough extending from the lower lakes to western Texas. During this time the barometer was slightly above 30 inches along the coast of Oregon and Washington, with generally cloudy weather and a well defined movement of the atmosphere from the Pacific coast across the divide toward the area of low pressure. Cloudless weather prevailed over the central slope on the 1st and 2d.

1890, *August* 6 *to* 8, *Alton, Kansas.*—"August 6 and 7, hot winds and high temperature; clear at night; 6th, maximum temperature, 102°.8; minimum, 74°.5; 7th, maximum, 109°.5; minimum, 72°.5." A. H. Goddard, voluntary observer.

1890, *August* 6 *to* 8, *Cunningham, Kansas.*—"August 6 and 7, warm winds; 8th, hot day and hot winds." E. Shaw, voluntary observer.

1890, *August* 6 *to* 8, *Gove, Kansas.*—"August 7, very hot day; high south wind; very hot and very oppressive." Jesse Rogers, voluntary observer.

This period of hot winds accompanied an area of low pressure which appeared to the northwest of Montana on August 4 and moved southeastward along the eastern slope, reaching the northern portion on the 6th and the central portion on the 7th, after which it moved rapidly to the northeast. During this time there was an area of high pressure in the Pacific off the coast of Oregon, with cloudy weather over the Pacific coast, and there was a well defined movement of the atmosphere across the divide toward the low pressure area. Generally cloudless weather prevailed over Kansas and Nebraska from the 6th to the 8th.

1890, *August* 12, *Cunningham, Kansas.*—"August 12, hot winds all day." E. Shaw, voluntary observer.

These winds accompanied an area of low pressure which appeared to the north of Washington on August 8 and

moved slowly southeast along the eastern slope, reaching the central portion on the 12th, in a V shape, extending from Texas to the northern boundary of the United States, with a distinct low pressure area developing over northern Kansas and southern Nebraska. On the 13th this depression was completely filled up. As the low pressure advanced from the north of Washington southeastward an area of high pressure advanced slowly from the Pacific, reaching the northern and central plateau regions on the 12th. The general tendency of the atmosplrere from the 8th to the 12th was across the divide toward the area of low pressure. Cloudless weather prevailed over Kansas on the 12th.

1891, *August*, *Independence*, *Kansas*.—"This month was the dryest on our record, and, following a light rainfall for July, was severe on our crops. A modifying effect was a low average temperature." J. M. Altafer, voluntary observer.

1892, *May* 29 *to* 30, *Mesquite*, *Texas*.—"May 29: Hot currents of wind commenced about 5 p. m. These currents were from 100 feet to 200 yards or more in width and came in rapid succession from the south. Sometimes three or four of these currents could be seen at once, going parallel with each other. These currents were wider to the west of this place than they were in this immediate vicinity. In some instances as much as eight acres of cotton were killed in one place, and in a number of smaller streaks, amounting to two and three acres in each, the cotton was burned up. Some of these hot currents would almost stop one's breathing. I was caught in the center of one about 100 feet in width and it was almost insufferable. At the appearance of these hot blasts the wind veered from southeast to south and clouds began to form in the southwest just above the horizon. The formation and disappearance of clouds was very rapid. Between 6:15 p. m. and 6:30 p. m. the wind veered to southwest and west, reaching the velocity of a strong gale, breaking off the limbs of trees. There was no visible electricity until about 6:30 p. m., when lightning was constant in the south and southwest. Light rain fell between 6:30 p. m. and 7 p. m.—amount, .08 of an inch—and still less fell in the localities where these hot currents had ceased only a few minutes before." Silas G. Lackey, voluntary observer.

1892, *May* 29 *to* 30, *Arlington*, *Texas*.—"Memoranda of hot winds May 29, 1892:

Central time.	Temperature (F.).	Remarks.
5:20 p. m.	89°	Wind southwest and south.
5:30 p. m.	96°	
5:39 p. m.	90°	
5:43 p. m.	88°	
5:50 p. m.	87°.5	First thunder. Wind southeast and east.
5:55 p. m.	87°	Wind south.
6:25 p. m.	71°	Light sprinkle at 6:15 p. m.

"These are observed readings: Wind high—possibly thirty miles per hour. No visible effect on vegetation here, but about three miles west a few acres of cotton are said to have been killed by the hot winds." C. F. Mercer, voluntary observer.

1892, *May* 29 *to* 30, *Sulphur Springs*, *Texas.*—"The hot, dry winds of May 29 and 30 passed over this (Hopkins) county. They came from the south about 9 p. m. In some parts of the county only the very tops of plants were scorched; in other parts the whole plants were scorched black. The winds did not move in a continuous line, since they scorched only in patches, sometimes twenty feet wide and sometimes more. Vegetation in the path of the wind looked as if it had been burned by fire. As nearly as I can learn, the winds covered about one-fourth of the county. The crops in the eastern and southern portions suffered most." H. A. Evans, voluntary observer.

1892, *May* 29 *to* 30, *Black Jack Grove*, *Texas.*—"On the night of the 29th, about 10 o'clock, a high, warm wind blew from the southeast. Considerable destruction was done to growing crops and vegetation generally in streaks over the fields—say from two hundred yards to one-fourth of a mile wide, in zigzag ways. There was a great deal of lightning in the southwest, but no rain at all." W. H. Davidson, postmaster.

1892, *May* 29 *to* 30, *Eastland*, *Texas.*—"On May 29, between the hours of 2 p. m. and 4 p. m., hot winds were felt in all parts of this (Eastland) county. They came from the south, lasting from two to five minutes. The effect on vegetation was of a scorching nature, but no serious damage was done except to gardens." J. A. Feddeman, voluntary observer.

1892, *May* 29 *to* 30, *Anson*, *Texas.*—"The hot winds have ceased to be a new phenomenon here, between the foot of the plains and the Colorado mountains, as they are of annual occurrence, during the month, generally of June (sometimes earlier, and others extending into July). The hot wind is the crucial test on our corn crop, and for that reason it is considered the most unreliable crop here. The hot wind usually comes from the north or northwest and is very hot and dry. Crops wither; the flesh becomes dry and very hot, with no moisture on the skin. In eleven years' residence here we have had but one or two seasons without these winds. We expect them yearly." C. Tompkins, voluntary observer.

1892, *May* 29 *to* 30, *Graham, Texas.*—"May 29 and 30, hot south winds; corn blades slightly burned." A. B. Gant, voluntary observer.

1892, *May* 29 *to* 30, *Roby, Texas.*—"Hot winds have occurred about every year since I have been in this section (six years). However, they have not always come at a time when they would damage crops, as they did on May 29 and 30, this year. They affected the wheat strangely. The crop was in bloom. The chaff opened out, exposing the germ of the kernel, just commencing to form. It did no more; never filled. They affected the crop in spots or streaks. These winds were from the southwest and commenced about 1 p. m. on the 29th." Hamilton Foster, voluntary observer.

1892, *May* 29 *to* 30, *Copeville, Texas.*—"On Sunday, May 29, hot winds were felt here, killing all vegetation with which they came in contact, covering territories of one-fourth of an acre to one acre. They came from the southwest." M. E. Campbell, voluntary observer.

1892, *May* 29 *to* 30, *Ben Franklin, Texas.*—"May 29 was warm, with wind about south-southwest. At about 12 noon (central time) there were occasional flashes of hot wind, but no damage resulted therefrom." S. D. Ross, voluntary observer.

1892, *May* 29 *to* 30, *Plano, Texas.*—"The hot winds of May 29 killed about ten acres of vegetation entirely dead in this vicinity. They came from the southeast about 6 p. m. The largest area touched in a body was six acres. The others were in spots about 25 feet wide and of various lengths." R. L. Livingston, voluntary observer.

1892, *May* 29 *to* 30, *Elkhart, Texas.*—"The hot winds touched this vicinity lightly. One or two farms were injured, apparently by something hot crisping the plants, but they may yet develop a slight yield of cotton." James W. Bryan, postmaster.

1892, *May* 29 *to* 30, *Haskell, Texas.*—"May 29, hot winds, but little damage done to vegetation except corn, the tops of which were considerably parched by the heat. The winds came from a southwesterly direction at about 2:30 p. m." H. R. Jones.

1892, *May* 29 *to* 30, *Mount Vernon, Texas.*—"The hot winds struck the prairie lands about fifteen miles from here, in Franklin county. The timbered sections did not experience any damage." J. W. Jordon, postmaster.

1892, *May* 29 *to* 30, *Rockwall, Texas.*—"May 29, hot, dry winds from south and southwest about 3 p. m.; very little damage done to crops." R. E. Chandler, voluntary observer.

1892, *May* 29 *to* 30, *Pilot Point, Texas.*—"May 29 we had dry, hot winds from the southwest, but not enough to injure crops." D. A. Wellborn, postmaster.

1892, *May* 29 *to* 30, *Round Rock, Texas.*—"May 30 was sultry and wind from south, and some mention was made of the peculiar heat accompanying the same, but no damage to vegetation other than withering of plants." Thos. Wood, voluntary observer.

1892, *May* 29 *to* 30, *Brownwood, Texas.*—"Hot winds May 29 and 30. An experienced farmer tells me that they curled the corn badly, and that if they had continued for two or three days longer corn and cotton would have been badly damaged." Prof. C. S. Newhall, voluntary observer.

1892, *May* 29 *to* 30, *Paint Rock, Texas.*—"The hot wind came from the southwest about 4:30 p. m. on May 29, and on next morning it was from northwest, and both just dried our corn." J. G. Sherwood, postmaster.

1892, *May* 29 *to* 30, *Gainesville, Texas.*—"In reply to yours asking about hot winds on May 29 and 30 ult. will say that after the heavy rain of May 31 the cotton crop of this county was found to be in a greatly damaged condition, a large per cent. of the plants being dead and the leaves killed on much larger per cent., which was not entirely dead. No one was able to account for this, and it was possibly the result of hot winds, although no one observed such winds, but the 29th and 30th were very hot days. Much of the cotton crop is so badly damaged that it has to be replanted." D. F. Ragsdale, voluntary observer.

The above embraces all the points from which reports of hot winds of May 29 and 30, 1892, could be obtained, but a number of negative reports were received from points within the affected district, which are given in order to show that the winds were not general.

Henderson, Texas.—"Heard of the hot winds, but did not experience them in this section." J. M. Hicks.

Greenville, Texas—"No hot winds on dates mentioned." H. Wagner.

Longview, Texas.—"No hot winds." G. W. Krech.

Mount Vernon, Texas.—"Have had no hot winds here." G. E. Cowan.

Kemp, Texas.—"On May 30th we had hot weather, but no hot, dry wind that damaged growing crops." J. E. Galey, postmaster.

Winnsboro, Texas.—"The hot winds did not strike this county." T. J. Gibson, displayman.

Forestburg, Texas.—"No hot winds here." J. N. Morris, voluntary observer.

Throckmorton, Texas.—"No hot winds." C. B. Hester.

Granbury, Texas.—"No special hot winds on the dates mentioned. Hot winds sometimes come from the southwest and parch vegetation in one day." S. J. Thrash.

Henrietta, Texas.—"We had hot weather the last of May, but no damage to vegetation on account of hot winds." C. B. Patterson.

Grapevine, Texas.—"Did not have hot winds on the dates mentioned." W. J. Crowley, voluntary observer.

Pittsburg, Texas.—"The weather was very warm, but no phenomena of an unusual nature." D. B. Smith.

Terrell, Texas.—"No hot winds so far as I can learn." E. Johnson.

Glen Rose, Texas.—"We had no hot, dry winds." C. J. Chandler.

Brady, Texas.—"The 29th was unusually hot, but no hot winds. No appreciable damage done to crops." W. H. Potter, voluntary observer.

Hillsboro, Texas.—"Hot winds did not touch this section." E. W. Booth.

Gatesville, Texas.—"No hot winds on dates to which you refer." A. D. Honeycutt.

Baird, Texas.—"We did not experience hot winds on dates mentioned." Wm. McManis, postmaster.

Linden, Texas.—"No hot winds in this section." C. A. Kesseler, postmaster.

San Saba, Texas.—"No hot winds here on May 29th and 30th." H. S. Chamberlain.

Albany, Texas.—"No hot winds on May 29th and 30th. Our hot winds come from southwest or west." N. L. Bartholomew, voluntary observer.

Paris, Texas.—"No hot winds." J. F. Hooks, voluntary observer.

Bonham, Texas.—"We had no hot winds on dates mentioned." M. R. Kimmins.

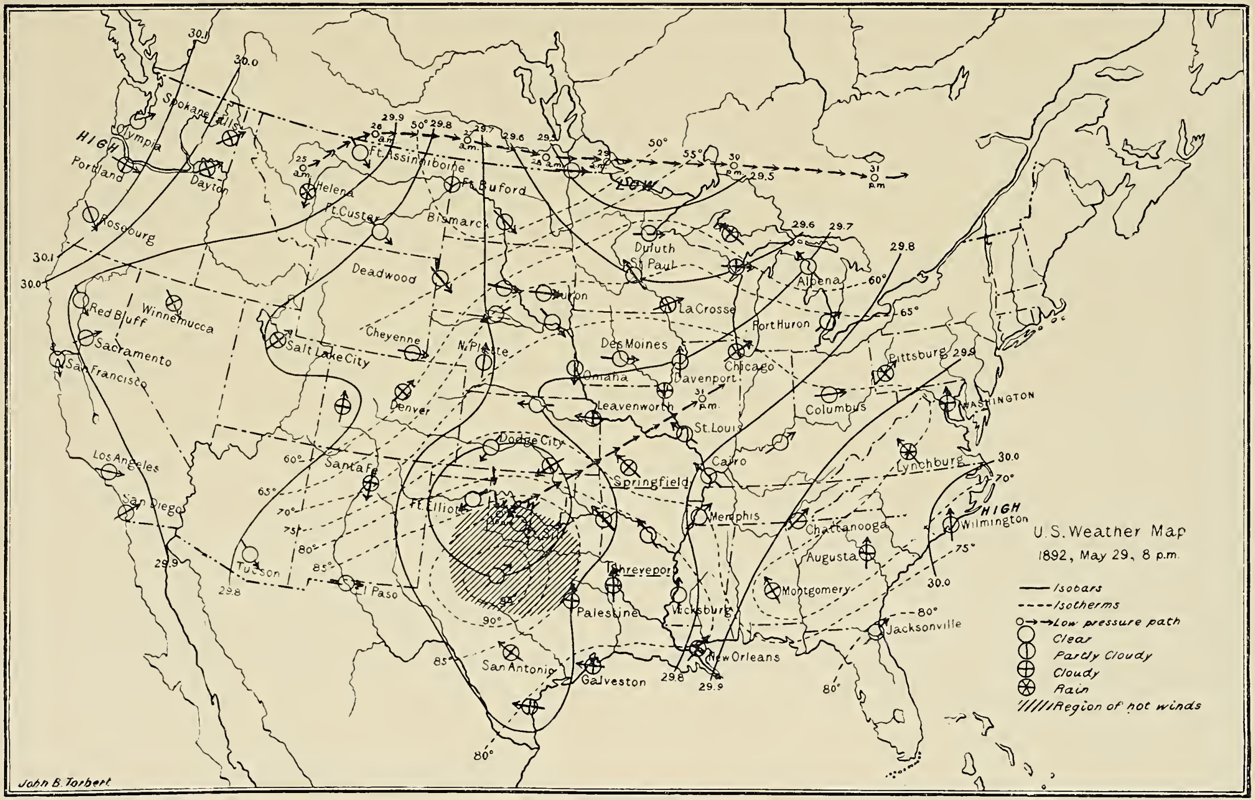
Palo Pinto, Texas.—"No hot winds so far this season." J. W. Schoolevalt.

Decatur, Texas.—"No hot winds in this section on dates to which you refer." W. M. Barber, postmaster.

The hot winds of May 29th and 30th, 1892, occurred with an area of low pressure, which made its appearance over northwestern Montana on the 25th and moved to the north of Montana on the 26th, when it developed into a **V** shaped low pressure area which covered the eastern slope from the British possessions to western Texas. The low pressure moved slowly toward the east and deepened southward until the 29th, when a well defined secondary low pressure area developed over the southeastern slope and remained nearly stationary until the afternoon of the 31st, when it moved off to the northeast. During this time an area of high pressure was in the Pacific off the coast of Oregon and California, and there was a well defined movement of the atmosphere from the Pacific coast across the divide toward the area of low pressure, with general rains to the west of the divide. (See plate 6.)

1892, *August* 6 *and* 7, *Altoona, Kansas.*—"August 6 and 7, hot winds." Wm. Cheetham.

1892, *August* 6 *and* 7, *Cunningham, Kansas.*—"August 6, an almost un-



CLINE. HOT WINDS ON THE GREAT PLAINS.

bearable hot south wind; wind veered from south to northeast at 7 p. m. Hot winds from 1st to 8th, inclusive." E. Shaw, voluntary observer.

"Newspapers report considerable damage from hot winds during the first ten days of August." I. M. Cline.

These hot winds were preceded and accompanied by the following conditions: An area of low pressure made its appearance to the north of Idaho on July 31 and moved eastward and southward, covering the northern and central slopes and the upper Missouri valley on the 2d of August. On the 3d this low pressure had moved out over the lakes, a high pressure area had appeared over the upper Missouri valley, and another low pressure was developing to the north of Montana, which extended southeastward to the central slope and moved slowly eastward, passing to the north of the lakes on the morning of the 6th. This low pressure area was followed by another, which developed in the vicinity of Calgary on the 5th, moved southeastward and covered the northern and central slopes during the 6th, 7th, and 8th, after which it moved to the north of the lakes. During the development and progress of these low pressure areas the barometer was above 30 inches over western portions of Oregon and Washington, and the general movement of the atmosphere was well defined from the Pacific coast across the divide toward the low pressure areas. The weather was generally cloudless over Kansas and Nebraska from August 1 to 8, inclusive.

General Characteristics of the Summer Hot Winds of the Great Plains, and Conclusions as to their Causes.

One of the most striking features of these winds is that while the atmosphere is heated generally and shows an excess of temperature over the territory affected, abnormally heated narrow currents are often observed between which the air is much cooler. These currents are of very short duration, but often occur in rapid succession at neighboring

places. These hot currents occur in groups, covering, as a rule, a territory of a few hundred acres. The currents are separated from each other by only short distances, ranging from a few yards up to a few hundred yards. Sometimes groups of hot winds separated by only a few miles cover several counties, and again several miles intervene between such groups. In this manner an occurrence of these winds sometimes covers the whole or part of a State or two or three States.

The tremor of heat, similar to that seen rising from a hot furnace, is sometimes distinctly visible in these currents. Observers who do not note the distinct currents, as well as those who record this feature, refer to these winds, as a rule, as "hot winds," and not a hot wind. We have no definite record of the temperature of the "hot winds"* themselves, but the thermometers in the affected district range generally, during the prevalence of these winds, from 100° to 110° in the shade, and at times even higher. During hot winds in Texas July 18, 1886, the thermometer at Abilene registered 109.8° in a standard roof shelter of the Signal Service. The temperature in the sun was taken by myself and others, and 140° was recorded at a short distance from the office instruments, with wind blowing from seven to fourteen miles per hour from the south, except at 1:21 p. m. (local time), when it was southwest. During hot winds in Texas May 29, 1892, a rise in temperature of seven degrees was noted between 5:01 p. m. and 5:11 p. m. (local time), an hour of the day when the temperature should have been falling. The local and special hot winds (hot currents), as noted in connection with a general hot wind, is a characteristic which I do not find to have been noted in connection with the warm winds of other regions in this country or elsewhere, notably the foehn, the chinook, the simoom,

*The term "hot winds" in this memoir will refer to the intensely heated currents, while the term "general hot winds" will refer to the movement of the generally heated atmosphere prevailing with such hot current.

etc.; and this feature of the hot winds in summer over the eastern slope of the Rocky mountains had been overlooked in previous discussions.

The hot winds are likely to occur between May 15 and September 15, but are most frequent during July and August, and the thirty-five periods of which we have records are distributed as follows: May 1, June 4, July 16, August 13, and September 1. The length of time during which a period of these winds is likely to prevail varies from a few hours to three days. Sometimes, but rarely, two or three of these periods of hot winds follow each other in rapid succession. Some observers have only noted these winds during the day; others have noted them continuously for twenty-four hours, and even longer, while others have observed or rather experienced them only at night, and they have often been noted as continuing into the night. During the hot winds in Texas July 18, 1886, the temperature at 9:21 p. m. (local time) stood at 96°, with wind eight miles per hour, from the south.

The hot winds are always referred to as extremely dry. Although very few hygrometric observations have been made in connection with them, yet those on record confirm the general statement regarding this condition. Prof. F. H. Snow, of the University of Kansas, recorded a relative humidity of 7 per cent. at Lawrence, Kansas, and the observer of the Signal Service at Leavenworth, Kansas, recorded at 5:48 a. m. (local time) 90; 9:48 a. m., 52; 1:48 p. m., 17; 5:48 p. m., 17, and 9:48 p. m., 22 per cent., September 12, 1882. On September 13, 1882, the observer at Leavenworth recorded the relative humidity at 5:48 a. m. (local time), 45; 9:48 a. m., 41; 1:48 p. m., 20; 5:48 p. m., 14, and 9:48 p. m. 20 per cent. On the 14th, 5:48 a. m., 50; 9:48 a. m., 25; 1:48 p. m., 13; 5:48 p. m., 18, and 9:48 p. m. 44 per cent. On the 15th, 5:48 a. m., 63; 9:48 a. m., 46; 1:48 p. m., 29; 5:48 p. m., 28, and 9:48 p. m. 51 per cent.; after which there was as decided an increase in moisture as there was a decrease after the 11th and the morning of the

12th. The observer of the Signal Service at Dodge City recorded the relative humidity on September 11th, 5:27 a. m. (local time), 48; 1:27 p. m., 14; 9:27 p. m., 24 per cent., which is about one-half the per cent. recorded on the preceding date· On the 12th, 5:27 a. m., 32; 1:27 p. m., 12; 9:27 p. m., 47 per cent. On the 13th, 5:27 a. m., 48; 1:27 p. m., 15, and 9:27 p. m. 48 per cent. On the 14th, 5:27 a. m., 37; 1:27 p. m., 11, and 9:27 p. m. 66 per cent. The last observation of the day shows a sudden and decided increase in moisture, which continued and was as marked as the decrease from the 10th to the 11th. During hot winds in Texas July 18, 1886, the relative humidity recorded at the regular observations at different times during the day was as follows: 5:21 a. m. (local time), 39; 9:21 a. m., 20; 1:21 p. m., 16; 5:21 p. m., 16, and 9:21 p. m. 18 per cent., at Abilene, Texas. The above records of the moisture in the generally heated atmosphere were made without reference to the occurrence of the special hot winds; hence the per cent. of moisture within these must approximate closely to zero.

The records of stations near localities from which hot winds have been reported have been examined for several periods of such winds and the conditions are found in all cases examined to be similar to those noted in connection with the above stations.

The direction from which the hot winds blow is the same as that of the atmosphere near the earth's surface prevailing over the section in which they occur at the time of their occurrence, and is generally southwest or south, sometimes southeast, and frequently from a northerly direction in some sections—particularly the extreme western portion of Texas, as the conditions under which hot winds occur in that section give northerly surface winds. The velocity of the hot winds varies considerably in different periods. It is sometimes noted as a light breeze and from that to a gale. It is not the same in all sections on the same date; while it is blowing a gale at one place it is noted as a light breeze at another place less than one hundred miles distant.

The region of the atmosphere in which the hot winds occur has a well marked progressive movement from west to east—at least, this is found to be the case whenever the data have sufficed to afford an opportunity to look for this feature. The dryness attending the hot winds in Kansas September 11 to 14, 1892, appeared one day earlier at Dodge City than at Leavenworth, and ceased one day earlier at the former place than at the latter. The eastward progression is also clearly shown by the reports of hot winds in Texas May 29, 1892, where they can be distinctly traced from the western portion of the State, where they appeared about noon, to the eastern portion, where they were not noted until as late as 10 p. m.

A striking characteristic of the hot winds is their effect on vegetation. While they are always noted as causing vegetation to wilt and droop, the more intensely hot winds burn tender vegetation to a crisp in a few minutes, without relation to the amount of moisture present in the soil or general atmosphere. Some of the most destructive of these winds have been known to occur when both the soil and atmosphere were saturated with moisture. The tops of corn and other hardier vegetation are burned, while near the earth they are not damaged. The leaves of trees dry to a crisp in some instances so that they crumble at the touch of the hand, and apples bake on the trees. Corn when in silk and wheat when in dough suffer more severely than at any other stage of their growth. Wheat in this stage when affected suffers severely. The heat and rapid evaporation completely dry up the germ wherever the hot winds strike. Fortunately severe damage from hot winds is not general over an affected district, but covers only small parts of the different farms. Sometimes entire counties remain uninjured. Under the influence of these winds the skin becomes dry and parched and perspiration becomes entirely insensible. The more intensely heated currents are said to be almost insufferable. When a period of hot winds continues for two or three days and the general atmosphere be-

comes very hot and dry vegetation suffers generally, but as a rule it recovers, except in those streaks visited by the intensely heated currents, although the yield is reduced somewhat and is reduced materially if two or three of these periods follow each other in rapid succession, as has sometimes been the case.

Prof. J. T. Lovewell,* speaking of the damage to crops in 1887–'88, says: "During both these years we heard many "complaints of blighted crops, but the cause was not wholly "a dearth of rain. When the hot period of July came dur-"ing the last season (1888) it was accompanied by a high "scorching southwest wind which prevailed for several "days, sweeping from the southwest corner up through the "middle of the State and cutting short the corn crop through "the breadth of several counties. These hot winds contrib-"ute more to the failure of crops in our State than does the "lack of moisture in the soil."

In studying the distribution of pressure in connection with hot winds it is found that they occur mostly with low pressure areas which have moved slowly from the north of Montana southeasterly along the eastern slope for three or four days before they take up a decided movement eastward. They sometimes accompany a low pressure area which develops over some part of the eastern slope and remains nearly stationary or moves slowly north or south along the eastern slope for a few days before moving off, and occasionally they accompany low pressure areas which have moved slowly across the divide from the Pacific coast and then move slowly north or south along the slope before moving off eastward. Of the thirty-five periods of hot winds twenty-eight occurred with low pressure areas which have moved southward from northwestern Montana. One accompanied a low pressure area which moved from the Pacific coast across the divide to the central slope, and the remaining six developed over some part of the eastern slope.

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* *In* Sixth biennial report of the Kansas state board of agriculture. 8°. Topeka, 1889, part 2, p. 214.

The low pressure area accompanying twenty-six of these periods was of an elongated or elliptical form and extended south from Northwest Territory, generally to the thirty-fifth parallel of latitude and in some instances to western Texas. These may be described as V shaped. In nine of these there were secondary depressions in the vicinity of the hot winds. In the other nine periods of hot winds (not included in the above) there was a general low pressure area along the eastern slope, and this sometimes extended eastward to the lakes and central valleys. In four of these there was a general well defined cyclonic movement, while in the other five there was a well defined secondary low pressure in the locality in which the hot winds occurred. During the development and progress of all these low pressure areas the barometer was generally about 30 inches or above along the western coast of the United States between the thirty-fifth and forty-eighth parallels of latitude. The eastward movement of the atmosphere from the Pacific slope across the divide toward the low pressure area was generally well defined, and cloudiness, with more or less rain, prevailed from the Pacific coast to the summit of the Rocky Mountain divide. The hot winds are generally found near the innermost or the second isobar of the low-pressure area which they accompany, and their direction, like that of the surface winds, conforms to the trend of the isobars.

The existence of a particular class of winds in summer over the eastern slope of the Rocky mountains or any part of it has been denied by some who have studied the subject and who claim that the term "hot winds" as used by the inhabitants of that section applies to winds which occur under the same conditions and are of the same generic character as the warm wave or heated term of the eastern States, and that their dryness and heat are caused by drouth and *insolation* * at the earth's surface. This view is strongly

* The word "insolation" is used to imply local heating at and near the earth's surface by the rays of the sun, including absorption. radiation, and convection.

supported by Mr. Curtis in his paper on hot winds, already cited. On the other hand, in studying the detailed reports of these hot winds we find many features for which drouth and insolation will not account, and it appears evident that we have here a well defined and peculiar class of winds similar to the chinook* and foehn†, but differing in some respects from any winds previously noted. Conditions which will theoretically account for one case of these winds must be present in other cases where such winds are well defined. The theory that these winds are the result of drouth and insolation will not account for certain intense periods of hot winds which have occurred when the earth was wet and the soil full of moisture, and others following some of the wettest seasons that the section over which they occurred had ever experienced. Neither will this theory account for the narrow intensely heated currents often found in the general hot winds; neither will it account for the numerous occurrences of such winds at night, and, further, it will not account for the extreme dryness which is noted in the majority of instances. Furthermore, if these winds were the result of drought and insolation merely, they would not depend on the existence and position of low pressure areas, but would be found prevailing in clear, dry weather whenever severe drought conditions exist. These winds are only found, however, with certain low pressure areas, and, further, during some of the dryest periods on record, their absence is noted.

In popular opinion the hot winds have been classed with those which carry with them in their progress northward the heat of the climate in which they are supposed to have had their origin; but an examination of the surface conditions prevailing to the south of the hot winds shows that a greater per cent. of moisture is always found to the south of them, and that as a rule the general temperatures

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* Described by Prof. M. W. Harrington *in* American Meteorological Journal. 8°. Ann Arbor, Mich., vol. 3; 1886, Nov., pp. 330-338; 1887, Feb., pp. 467–475; Mar., pp. 516–523.

† Described by Dr. J. Hann *in* Handbuch der Klimatologie. 8°. Stuttgart, 1883, pp. 208 et seq.

are lower to the south than over the territory affected by these winds. Therefore general southerly winds do not explain the origin of the special hot winds. Such winds would, moreover, be general, and not confined to the narrow limits often described in connection with these hot winds. Furthermore, this explanation would not be applicable to those hot winds which blow from the north.

The opinion that the general hot winds over the eastern slope of the Rocky mountains in summer are a special class, and that their heat and dryness are of dynamic origin the same as that of the foehn and chinook, has been advanced by a few scientists, and the present study of all the observations and correlated facts which can be found relating to this subject certainly appears to confirm the theory that these winds are of such origin. The distribution of pressure in each case where these hot winds have been noted is such as would augment the generally eastward movement of the atmosphere of the middle latitudes. The high pressure off the coast of Oregon and Washington tends to give southeasterly winds between the coast and the divide, and this force acting with the general trend of the atmosphere of these latitudes causes the winds to become more easterly. With these conditions it can readily be seen that an area of low pressure remaining nearly stationary over the eastern slope of the Rocky mountains for a few days would cause the wind to continue its course across the divide toward such low pressure area. The atmosphere as it moves eastward from the Pacific coast to the crest of the continental divide is always accompanied by clouds and more or less rain, and at the outset is nearly saturated with moisture. In crossing the divide to reach the low pressure area this air must ascend to a height of 10,000 to 15,000 feet. From the deductions of Dr. Hann,* Prof. Bezold,† Prof.

* The Laws of the Variation of Temperature in Ascending Currents of Air, etc. Zeitschrift Oest. Met. Gesell., 1874, pp. 321-346, IX.

† Thermodynamics of the Atmosphere in the Sitzungsb. Ak. Wiss., Berlin, 1888, pp. 485-522.

Ferrel,* and others we have learned that saturated air while ascending cools at the rate of about one degree Fahrenheit in each four hundred feet elevation † instead of one degree in about one hundred feet, as is the case with dry air. The reduction of temperature in ascending moist air is compensated about one-half by the heat liberated in the condensation of moisture resulting from cooling in the ascent. According to Dr. Hann,‡ one-half of the vapor of the atmosphere is below an elevation of 6,000 feet and eight-tenths is below 15,000 feet; hence we can readily see how small an amount of moisture can be retained by the atmosphere after crossing a mountain range with an altitude of 10,000 to 15,000 feet. The Central Pacific railroad officials have kept records of the precipitation along the main line of their road from Sacramento to the summit pass for several years. On the result of these records it is stated § that the annual precipitation increases at the rate of one inch for every one hundred feet altitude; that at the summit pass the mean annual precipitation exceeds ninety inches, and that it is not improbable that this amount is considerably exceeded along the crest of the range. Eight tenths of the moisture of our atmosphere is below the crest of those mountains, and air in passing over them loses a large percentage of this moisture. This dry air, in descending over the eastern slope, after having dissipated the cloud carried over, gains temperature dynamically nearly twice as rapidly, in a corresponding distance, as it cooled in ascending the western slope. In moving toward the low pressure area this dry air takes up the circulation around that area, is carried over

* Recent advances in meteorology; being Appendix 71 to Annual report of the Chief Signal Officer. 8°. Washington, 1886; part 2.

† This is the rate when pressure = 30 inches and temperature = 67°; or p = 25 inches and t = 60°; or p = 20 inches and t = 53°.

‡ Distribution of Aqueous Vapor of Atmosphere with Increase of Altitude. Zeitschrift Oest. Met. Gessell., 1874, pp. 193 to 200, IX.

§ By Capt. C. E. Dutton, *in* American Journal of Science. 8°. New Haven, 1881; October; no. 130; 3d series, vol. 22, p. 248.

the plateau region from a northerly direction, and flows down over the eastern slope from a westerly and then a southerly direction, depending on the trend of the isobars. The mass of air, in moving down the slope, loses a great deal of the warmth derived dynamically, by radiation to the earth and the surrounding atmosphere, and also by intermixture, and it reaches the lower altitudes with dryness and increased temperature, but with less warmth than if the descent had taken place rapidly. The dry air is carried forward in the upper strata more rapidly than in the layers near the earth's surface, and when thus carried out over moister and less dense air its tendency is to descend here and there through that air to the earth's surface, while the moist and less dense air ascends at neighboring points and forms the scattering clouds often noted in connection with these winds. Similar descending currents, on a small scale, have often been observed by the writer to descend on dusty roadways and blow the dust out in all directions, then move off a short distance with the surface wind and die out. These currents (or masses of air) in descending rapidly from great elevations gain a great deal of warmth, and reach the earth with their initial dryness. In order, however, to become so intensely hot they must descend rapidly or the warmth gained dynamically is lost by radiation. After the earth's surface is reached, both radiation and intermixture become rapid, and each individual current, except in the case of a very large one, is of short duration. This explanation accounts both for the general heating of the atmosphere and for the intensely hot currents, and also accounts for the occurrence of these hot winds at any time of the day or night, from any direction, and without regard to the conditions of the soil, whether wet or dry.

In these descending currents or masses of air evidently very little intermixture with the general atmosphere takes place until the earth's surface is reached. They appear to retain their identity with sharply defined boundaries, to reach the earth with almost extreme dryness, and with

warmth gained dynamically when the descent has been rapid. Horizontal layers or currents of dry or moist, hot or cold air interposed upon each other with the lines dividing them sharply defined have been noted by observers in balloon ascensions, and the study of these phenomena in this investigation appears to demonstrate that vertical currents may have as sharply defined boundaries, so far as their conditions in relation to the general atmosphere are concerned, as have been observed in connection with horizontal currents.

These hot winds are more frequent between the 34th and 45th parallels of latitude than to the north or south of this territory. This results from the southern portions of the V shaped depressions resting generally over this section. The conditions with which hot winds generally occur over this section are shown in plate 4. The hot winds to the south of the 34th parallel occur with the development of secondary low-pressure areas, as represented by plate 6. Those to the north of the 46th parallel accompany conditions similar to those which produce the chinook in winter, namely, the low pressure to the north of the Dakotas and Minnesota, with the high pressure over the northern plateau region and extending back toward the Pacific coast, as represented by plate 5. The conditions which produce the hot winds over the last two sections of the territory named are more rare than the conditions which produce them over that portion between the 34th and 46th parallels of latitude, hence hot winds are not frequent over these sections.

Winds similar in some respects to those general warm winds which occur over the eastern slope may be experienced with a less degree of heat, but intensely dry over the central valleys and even occasionally eastward on the Atlantic seaboard. Such winds are likely to occur when there is a high pressure area over the gulf and south Atlantic States, another along the Pacific coast, and two or three low pressure areas slowly follow each other in succession

from the eastern slope eastward over the lakes and down the Saint Lawrence valley. Low pressure areas, when moving very slowly and accompanied by brisk to high winds, will, when the high pressure areas occupy the positions referred to above, draw the atmosphere after them from some distance in the rear, while those moving eastward rapidly would soon leave the atmosphere, crossing the divide uninfluenced by their action.

Apparently the conditions on which the development of hot, dry winds over the eastern slope of the Rocky mountains and eastward depends are the presence of nearly stationary or slow moving low pressure areas along the eastern slope and then eastward, with a relatively high pressure over the Pacific off the coast of Oregon or in that vicinity. In no instances are hot winds noted with a low pressure area which moves eastward with any degree of rapidity.

Although the development of these hot winds is entirely independent of drought conditions, yet they will, of course, become much more intense, will extend over more territory, and be more injurious to crops when they occur during the prevalence of a drought than when they occur with seasonable weather, as vegetation in the former case will be the principal source from which they must draw their moisture, and the excessively dry winds can injure crops generally in a very short time. When the earth is moist crops are not likely to suffer seriously during the prevalence of a generally warm, dry wind, but principally suffer where the intensely hot winds occur, resulting from the rapidly descending currents; and the areas affected by these, while they make considerable show, are but a small amount when compared with the entire crop of a state. Occasionally one-sixth, or even one-fourth, of two or three counties are completely burned up, but such cases are rare. In fact, after a careful consideration of this subject, I have arrived at the conclusion that while these winds are often very intense and striking in their nature and damage crops to a considera-

ble extent, yet there are other sections of the United States where the farmer has as great drawbacks, to contend with, such as overflows, excessive rains, etc.

These winds are a feature of the climate of the eastern slope of the Rocky mountains, and cannot be expected to disappear or even become less frequent; neither are they likely to become more so, and, while nothing can be done to prevent their occurrence, steps should be taken to ameliorate their effects, if practicable. It appears that anything which would furnish an extended surface from which these hot and dry winds could absorb moisture, and thus by evaporation reduce their temperature as well as increase their moisture, would lessen their geographic extent and their injurious effects on vegetation. A generous growth of hardy timber appears to be the only material suggestion in this connection. If every farmer would hedge his farm and plant tracts of such hardy and long-lived trees as have been found to succeed in similar climates the injury resulting to crops from these winds might be appreciably decreased. Over much of the territory affected by these winds there is at present a scattering growth of timber, and this should be carefully protected and extended.

I desire to express my thanks to Prof. Mark W. Harrington and Prof. Cleveland Abbe for suggestions and references in connection with the preparation of this memoir.

RELIEF MAPS.

BY

Marcus Baker.

[Read before the Philosophical Society, February 3, 1894.]

Scattered among various buildings of the Columbian Exposition at Chicago were a goodly number of models or relief maps. Of these there were, in all, perhaps 75 or 100 of various sizes, from small to very large; of various colors—green, brown, yellowish-white, and assorted; of various degrees of exaggeration, from natural scale to more than forty-fold, and designed for various purposes.

Near the close of a brief visit to the exposition I began to take notes respecting these relief maps and to study them from the artistic or artisan's point of view. By this is meant that attention was given, not to the thing represented, but to the mode of representing the thing. For example, the relief map of the state of New York was examined, not for the purpose of learning something of the topography or geography of New York state, but to discover the qualities of the relief map. Did it give a true picture, or a pleasing one? What constituted its merits and its defects? And if a new model of the state were to be made, what could be learned from this model that would be helpful in making a better one? Up to the present time relief maps and relief-map makers are so few, at least in the United States, that competition or rivalry in their art has hardly begun. It would be highly instructive to bring together a collection of models for comparison and study, such comparison and study to be directed toward developing the art of relief map making.

Probably the largest number of such maps yet brought together were on exhibition at Chicago. Brought together must here be taken in a Pickwickian sense. To be sure they were in the White City, but it required several days to find and see them, and when found direct comparison was impossible. If they had been exhibited by the modelers or artists as samples of their work, they would naturally have constituted a group by themselves and been gathered in a single building. But this stage has not yet been reached; so the geographer interested in the art was forced to spend some time and care in searching out the examples of it on exhibition. Still the search was well worth making, since good relief maps are powerful aids in teaching, aids that are of value not only for the schools but for the public. Recognition of their value will, furthermore, stimulate their production, and with their increasing number it is in every way desirable that lofty ideals be conceived and striven for. Enough progress has already been made to reveal merits and defects and to suggest principles of general application—principles that, while still leaving many questions unanswered, will yet make possible the avoidance of certain grave defects and waste of time and energy.

Size of Relief Maps.—The relief maps exhibited in Chicago ranged in size from 4 or 5 square feet to nearly 1,000 square feet. Massachusetts, for example, with an area of about 8,300 square miles, was modeled on a scale of 4 miles to an inch, yielding a model $2\frac{1}{2}$ by 4 feet, or 10 square feet. The neighboring and slightly larger State of New Hampshire, with an area of 9,000 square miles, modeled on a scale of $\frac{1}{2}$ mile to 1 inch, yielded a model 15 by 31 feet, or of 465 square feet. Of Massachusetts we have a detailed topographic survey on a scale of 1 mile to 1 inch; of New Hampshire we have no detailed topographic survey. These two models, therefore, form an instructive contrast in the matter of size. Standing before and rather near the Massachusetts model, we may, without moving, take in the topographic relations of the whole State. Moreover, the topographic data used in its construction are sufficiently detailed and precise

to enable the modeler to correctly depict all parts. If striking peculiarities appear in any place they are not due to a lack of data, requiring that these places be filled in from imagination.

Standing before the New Hampshire model, we cannot take it all in at a glance; the eye wanders, or rather darts, from point to point, and in the end obtains not one clear, general impression, but a series of details. These details are not derived from detailed surveys nor from accurate data, and hence give crude or erroneous impressions. It is my opinion that the adoption of a smaller size for the New Hampshire model would have yielded a better result.

In criticizing this model I do not wish to be understood as criticizing the modeler. The modeler, like the architect, cannot always have his own way. He makes the thing ordered and paid for. When an individual or a commission or board orders a model made, conditions may be attached. Only when the modeler has absolute freedom in selecting his subject, size, scale and color scheme can he be held wholly responsible. When he is free to produce his ideal, then only will criticism have its full value. Indeed, the object of this and, as I conceive, of all art criticism is to reveal the ideal. It is only high-grade works that are worth painstaking and careful criticism.

The size of a map, whether plain or in relief, should be and is conditioned by the purpose for which it is made. If designed for use close at hand, as when held in hand, it should be small; if for use only at a distance it should be large, as in the case of wall maps intended for the use of audiences. In the case of relief maps their use appears to fall between the above extremes. They are usually exhibited lying on their backs on a table or raised platform and the user of them is permitted to come near. This mode of exhibition is a criterion for determining the ideal size. Let it be such that when so exhibited the eye can take in the whole without any movement of the head.

Most of the relief maps at the exposition fulfilled this condition. They were of such size as to be easily examined

and studied by one or several people at once and not so large as to destroy their unity. They could be grasped as a whole.

Two motives have here combined to produce reliefs of moderate size, viz., effectiveness and cost. Both of these tend in the same direction. Large models are expensive. From motives of economy, therefore, models of relatively small size have been the rule, and an examination of the very large models shows that this is a fortunate circumstance, since the comparatively small relief map is, other things being equal, the better one.

Vertical Exaggeration.—The vertical exaggeration in a model is the ratio of the vertical scale to the horizontal scale. Makers of relief maps have differed widely in the amount of such exaggeration. Formerly there was a tendency to exaggerate very much; the present tendency is toward the least possible exaggeration; and the questions which to me seem worth considering are, What degree of vertical exaggeration should be adopted in any particular case? What principles, if any, can be found that will aid in determining the appropriate exaggeration?

In endeavoring to discover such principles it is assumed that the model is made for the purpose of presenting a true topographic picture as accurate and as pleasing as possible. For general or educational purposes this is the object aimed at. We wish to show the surface form accurately and attractively. Flat country should look flat in the model and rough country should look rough. If a model of a flat State like Illinois shows a rough surface the picture presented by our model is not a true one. There is too great exaggeration. If a model of the mountainous state of Colorado looks flat or smooth too little exaggeration has been used. If we model Colorado with so much vertical exaggeration that its great mountain masses look like stalagmites these will not in anywise simulate the real mountain masses and will thus prove misleading. They are neither true nor attractive miniatures. There is too much vertical exaggeration. Pike's peak, Long's peak, and the Spanish peaks do not

look like inverted icicles as they have been represented in certain models designed for the schools.

The exaggeration of the vertical over the horizontal scale, needful for yielding a map which shall best express the topography, appears to be a function of two variables. These variables are the horizontal scale and the character of the country to be depicted. When the horizontal scale is large the area represented by the model will be small, and small details can be shown. We are nearer to nature with the large scale. Our maps are less generalized. Our model should, therefore, be less generalized and nearer to nature, for the true proportions of things shown should be presented. If we should model the Capitol building all will agree that it would be objectionable to use different scales for its length and height. Proportion would be destroyed. Should we make a miniature, not only of the Capitol, but of the surrounding parks and buildings, true scale relations should still be preserved. And this remains true if we should even include the entire District of Columbia. But when we embrace a yet wider area, as, for example, the entire States of Virginia and Maryland or the whole United States, still keeping the size of our model within moderate limits, we can no longer show details. The entire city of Washington is reduced so nearly to a point as to be shown merely by a convention, and the surface inequalities of the city and whole District fade out relatively to the whole. Only the great inequalities of surface can be truly shown. But precisely this is the object of a relief map covering a large area. It is to bring under a single view the great surface irregularities of an extensive area. This requires that we ignore small surface inequalities.

If we model a large area, and of course upon a small scale, we sometimes have a mountainous or very rough part and a plains or prairie area which, relatively to the other, is flat, even though on closer view considerable unevenness of surface appears. In such cases, shall we portray the mountainous parts in their true proportions and leave the small inequalities of the plains unrecognizable, or

shall we magnify the inequalities of the plains that they may be seen and then for uniformity exaggerate the mountain heights, or shall we adopt one scale of exaggeration for the broken and mountainous area and another for the plains area? Model makers have tried these three methods, so that actual examples may be seen and judged. The variable scale is open to the objection that true proportions are thereby destroyed. The model, being a unit designed primarily to show surface irregularity, will not truly show these relations if relatively flat areas have their small inequalities magnified.

I saw two illustrations of this change of vertical exaggeration in one model at Chicago, viz., New Jersey and Kentucky. The low relief of New Jersey's borderland between the seashore and the 100-foot contour was in the model exaggerated 33 times, between the 100 and 800 foot contour 16.5 times, and for the remainder of the State 11 times. As a result the model misleads as to the *relative* inequality of surface. Of the whole State two-thirds or more lies below the 200-foot contour and is very flat country. In order that the small inequalities and very gentle slopes which actually exist in this area might appear, a scale of exaggeration was adopted to bring them out. And it succeeded. They are brought out, but at the expense of accuracy and truth of the model as a whole. It seems to me, therefore, that the varying vertical scale does not successfully solve the problem of yielding the ideal map. Whatever scale be adopted for one part of the map, that scale should, in my judgment, be impartially used in all parts.

Two alternatives are now left. If we are to embrace within one map a district composed in part of rough or mountainous country and in part of comparatively smooth or level country, shall we exaggerate the vertical scale to show the minor inequalities of the relatively flat areas and so greatly distort the picture of our mountain masses, or shall we truly portray the great mountain features and sacrifice the flat country's detail, or shall we compromise by distorting both?

Now it is the existence of rough country that awakens our interest in and shows the need of relief maps. Such maps are primarily devised to aid in gaining a true perception of topographic relations in rough country. They are not needed for very flat areas unless for special purposes. The aim of the relief map is to give a comprehensive and true view of the large features over a wide area. We therefore act unwisely if we distort the picture of these great masses to save lesser details of relatively minor importance. If we are to model Colorado, let the vertical exaggeration be chosen with a view to correctly portraying her majestic mountains even at a total loss of the character of the small but interesting topography of the outlying foothills and plains. If this plains topography is to be shown in relief, let it be done by itself. In the relief map as well as in its forerunner, the topographic map, the less must be subordinated to the greater. In choosing our scale of vertical exaggeration let us therefore have it (*a*) uniform throughout and (*b*) selected with reference to the best exhibit of the great relief features.

If models of large areas and on correspondingly small horizontal scales are carefully compared with those of smaller areas on larger scales, it is found that an equal amount of vertical exaggeration in each produces unequal results. If the fitting exaggeration has been chosen for the small scale, then the large-scale model appears over exaggerated. For example, the model of the State of California, horizontal scale 4 miles to one inch, with a vertical exaggeration of 4, may be compared with the model of the Yosemite on a scale of two miles to an inch, with no vertical exaggeration. Or again, compare Maryland, horizontal scale two-thirds of an inch to a mile, vertical exaggeration 5, with the District of Columbia, horizontal scale 2 inches to 1 mile, vertical exaggeration 2. In each case the smaller area within the greater has been modeled on a larger horizontal scale and with diminished vertical exaggeration. An inspection of the models proves the wisdom of this choice and permits us to infer that, other things equal, large-scale

models require less exaggeration and small-scale models more. The nearer we are to our subject the smaller is the exaggeration required.

Again consider the subject to be modeled. If it is one of bold relief and large features, like the Yosemite, Grand canyon, or mount Shasta, these bold features need no exaggeration to be seen in their true relations. Indeed, in such cases vertical exaggeration will give a false impression. Hence, with bold features exaggerate but little. I believe a careful examination of the relief maps exhibited at Chicago will convince any one that the best ones are those which have either small or no vertical exaggeration. The makers of the models appear to have reached this conclusion, since examples of the excessive exaggerations, 15, 30, or even 50 fold, formerly used, were almost wholly wanting. Small or no exaggerations were the rule.

The following table classifies the relief maps shown at the exposition according to vertical exaggeration:

Exaggeration.	No. of models.
1	22
2	9
3	5
4	9
5	8
7	1
8	2
10	3
15	1
18	1
20	2
21	1
33	1
44	1
Variable	2
	68

Out of 114 models listed in the general table (p. 362–7) data respecting vertical exaggeration are complete for 68. This excludes models which differ in color only. Of these 68 models, 22, or 32 per cent., have no vertical exaggeration—they are modeled without distortion ; 34, or 50 per cent., have moderate vertical exaggeration, ranging from two to eight fold ; 10, or 15 per cent., have large vertical exaggeration, ranging from ten to forty-four fold, while 2, or 3 per cent., have a variable vertical exaggeration.

Most of our views of surface inequality have been obtained from points on the surface, points relatively low down. In this way the great mountain masses have been seen from their flanks rather than from their summits ; they have been seen in profile. Few of us have been favored with balloon views, and not many have enjoyed views from the highest mountain summits. Those who have enjoyed such views, as well as many others, have noticed the flattening effect produced by viewing a landscape from above. So viewed, all minor inequalities fade away and the country looks flat.

Now, the object of a relief map is to bring a considerable tract under view at once. This can only be done, either in nature or in a model, by viewing from above rather than from the side ; but to view from above is to lose the effect of ruggedness, of bold escarpment or yawning chasm. Hence modelers, whose work is perforce to be viewed from above, as from a balloon, which flattens the view, strive to give the effect produced from a much lower point of view by vertical exaggeration. It is a hopeless struggle. To present a view from one point which shall give the impression derived from a fundamentally different one has not been accomplished. I do not see how it ever can be. In nature it is the edge of the landscape, as it were, which is turned toward us as we view a wide expanse. The artist makes a model which is not to be so viewed, but from above, and then tries to preserve the effect of such side viewing. How near he will ultimately come to attaining this it seems idle to predict.

Coloration.—The attractiveness and effectiveness of a map is greatly influenced by its color or colors. All the models exhibited were painted. A mere inspection of a white cast tells us that without paint or color a model is unfinished—it is simply in process of making. So important a part does the color scheme play that an inferior model tastefully and truthfully colored is more attractive and pleasing than a much better one badly colored. The color schemes selected are realistic or conventional, or both, and are designed for topographic, for geologic, or for political purposes.

When we paint a model uniformly, whether brown, green, or any other color, irrespective of the colors in nature, we have a conventional coloring. When we paint the high and snow-clad summits a grayish white, the densely forested mountain slopes a dark green, the fertile foothill slopes in bright green, and the deserts in brown, with blue for all water bodies, we have a realistic scheme of coloring, and if to this we add the culture, the cities, roads, etc., in red, we have a scheme in part realistic and in part conventional. Examples of all these were on exhibition and upon me made the impression that for general topographic purposes the realistic scheme, or the realistic with conventional coloring for culture, gave the best results.

In the case of models showing geologic relations conventional coloring is a necessity. Bodies of color for political divisions on relief maps are to me even more distasteful than on plane maps.

Methods.—Two methods of exhibiting topographic relief were exhibited. In one the surface was modeled in some plastic material, as wax or clay; in the other the model was made by cutting out from copies of the same map, mounted on card-board, along successive contours, and then piling up these successive cuttings.

Most of the models shown were made by the first method. Judged merely by numbers, this is the approved method, but, irrespective of numbers, this method of showing topographic form gives the topographer an opportunity to ex-

hibit his appreciation of form and his skill in rendering what he sees. The second method is purely mechanical. The sides of smoothly sloping hills appear in the finished model as a series of steps, and the whole result looks wooden, stiff, and unfinished, like the new building from which the surrounding scaffolding, used during construction, has not been removed. The lesson of the Fair on this method is that its decadence is far advanced.

Purposes.—We have assumed throughout that the purpose of the relief map was to portray strikingly, truthfully, and artistically the relief forms shown. If such is not the purpose, then qualification is necessary. If the object is to advertise either a summer resort, a mineral spring, a canal project, or a boom town, other principles may be used for model-making, and in some, if not all, cases properly so. Visitors to the Mining Building were attracted by an enormous model of Nicaragua showing the proposed canal route. The topography of the model, with its 15-fold exaggeration, is a caricature, offending the eye of the topographer; and yet it doubtless served a useful purpose. It drew and for a brief interval impressed a few general features on the minds of the throng of sightseers, who might have passed a smaller, more artistic, and more truthful model unnoticed.

Of the model of the canal system of New York State, with its 44-fold exaggeration, this cannot be said. If Nicaragua was a caricature, this was a cartoon.

Those models which have been prepared for the purpose of misleading by deliberately making flat places steep or steep places flat or by any other departure from the known facts we pass by in silence. They are outside the purpose of this paper.

Installation.—One minor matter in connection with the exhibit deserves mention, to wit, the manner of placing or exhibiting the models. Most of them were placed flat, upon their backs, on a raised platform or table. Occasionally they were hung on the wall vertically and sometimes

were leaned back against the wall. When so placed as to be viewed from one position only they should be placed, if possible, with north to the top. Maps of California or Illinois or Massachusetts, when placed so as to be viewed from the east only instead of, as usual, from the south, make us feel the need of a translator. We recognize our familiar geography with difficulty when translated, as it were, into a strange tongue.

If the model can be viewed from all sides, then, if possible, let it be oriented truly.

Conclusions.

Relief maps designed to present faithful, striking, and artistic likenesses of topography should fulfill the following conditions:

1. *Size.*—They should be of moderate size, not exceeding five or six feet square.

2. *Vertical Exaggeration.*—The vertical exaggeration should be small, ranging from none at all up to 5 or 6 times, and should depend upon two things, viz:

(*a.*) Horizontal scale.

(*b.*) Character of the country modeled.

For large horizontal scales no exaggeration, for small horizontal scales moderate exaggeration.

For regions of bold features little or no exaggeration, for regions of low relief moderate exaggeration.

For regions comprising both bold and striking features and also low relief let the exaggeration be determined with reference to the large features only.

3. *Colors.*—The model should be colored and have for its relief features and drainage a realistic color scheme and for its cultural features a conventional color scheme.

4. *Installation.*—The model should be oriented in the usual manner, north at the top, and exhibited in the position taken by pictures on an easel.

The leading facts respecting various models exhibited at the Chicago Exposition are shown in the following table.

In compiling it, use has been made of the memoranda and notes gathered by myself, supplemented by data contained in the printed catalogues of Ward's Natural Science establishment, of Rochester, N. Y., and of Mr. E. E. Howell, of Washington. In addition to these Mr. Howell and Mr. E. E. Court have kindly furnished additional notes and memoranda, which have been used in preparing the table.

From the short time available for its preparation the table is sure to contain errors, and, from the nature of the case, is incomplete. As, however, no such bringing together of this class of facts has been heretofore attempted, it is believed that even this incomplete list will not be without interest.

Relief Maps at the World's Colum

Map.	Scales.			Size, inches.	Maker.
	Horizontal.	Vertical.	Ratio.		
	1 :	1 :	1 :		
United States	2,534,400	506,880	5	68 x 99	E. E. Howell
United States	2,534,400	506,880	5	68 x 99	E. E. Howell
United States	2,534,400	506,880	5	68 x 99	E. E. Howell
United States	2,534,400	506,880	5	68 x 99	E. E. Howell
United States	3,168,000	316,800	10	42 x 72	E. E. Howell
United States	2,500,000	250,000	10	48 x 81	Bürgi Brothers
United States and Alaska			1		D. B. Wainwright
Arkansas	190,080	24,000	7.9	84 x 93	J. C. Branner
California	253,440	63,360	4		W. D. Johnson
California	760,320	96,000	7.9	48 x 54	Bro. George Albert
Maryland	95,040	19,200	5		C. Mindeleff
Massachusetts	253,440	48,000	5.3	29 x 48	E. E. Howell
Pennsylvania	126,720	24,000	5.3		E. B. Harden
New Hampshire	31,680	6,000	5.3		C. Mindeleff
Kentucky	253,440	Various.	5 to 12.5	44 x 106	E. E. Howell
Kentucky	253,440	Various.	5 to 12.5	44 x 106	E. E. Howell
New Jersey	316,800	9,600 19,200 28,800	33 16.5 11	19 x 35	E. E. Howell
New York	63,360	6,000	10.5	324 x 444	H. L. Ward
Missouri	316,800	18,000	17.6	63 x 72	E. E. Howell
Illinois	126,720	6,000	21.1		Louise Barwick
Rio de Janeiro					
Drainage basin, Arkansas river	190,080	63,360	3	55 x 82	E. E. Howell
Twin Lakes reservoir, Colo	12,000	2,400	5	11 x 28	E. E. Howell
Irrigated fields				18 x 36	E. E. Howell
Uinta and Wasatch mountains.	253,440	126,720	2	48 x 50	E. E. Howell
Uinta and Wasatch mountains					
Grand Canyon district	126,720	126,720	1	72 x 72	E. E. Howell
Grand Canyon district	126,720	60,000	2.1	72 x 72	E. E. Howell
Henry mountains and vicinity.	126,720	126,720	1	36 x 42	G. K. Gilbert
Henry mountains and vicinity.	126,720	126,720	1	36 x 42	G. K. Gilbert
Henry mountains	42,240	42,240	1	43 x 60	G. K. Gilbert
Henry mountains	42,240	42,240	1	43 x 60	G. K. Gilbert
Henry mountains	42,240	42,240	1	43 x 60	G. K. Gilbert
Henry mountains	42,240	42,240	1	43 x 60	G. K. Gilbert
High plateaus of Utah	140,000	140,000	1	56 x 58	C. E. Dutton
High plateaus of Utah	140,000	140,000	1	52 x 53	C. E. Dutton
Mount Taylor, N. M	62,500	62,500	1	48 x 48	E. E. Howell
Mount Taylor, N. M	62,500	62,500	1	48 x 48	E. E. Howell
Yellowstone park	63,360	63,360	1	52 x 62	E. E. Howell
Yellowstone park	63,360	63,360	1	52 x 62	E. E. Howell
Mount Blanc, Switzerland	84,480	36,200	2.3	23 x 27	
Mount Shasta, Cal	30,000	30,000	1	39 x 40	V. and C. Mindeleff
Mount Shasta, Cal	30,000	30,000	1	39 x 40	V. and C. Mindeleff
Pike's Peak, Colo				48 x 60	

bian Exposition, Chicago, 1893.

Exhibitor.	Purpose.	Color scheme.	Remarks.
U. S. G. S.	Topographic	Water blue, culture red, land cream.	Section of globe of diameter 16½ feet.
National Museum	Geologic	Same. Ice-sheet bluish white.	Showing glacial ice-sheet.
Agricultural Dep't	Zoölogic	Conventional	Showing life zones.
Transportation Dep't	Transportation routes	Railroads in red	Showing railroad and steamship lines.
Ward's Nat. Sc. Est	Topographic	Water blue, culture red, land cream.	Section of globe 13.3 feet in diameter.
Ward's Nat. Sc. Est	School	Conventional for political divisions.	Section of globe.
C. & G. S.	Topographic		Built by contours; sph. surface.
	Topographic	Yellow	
California	Topographic	Realistic	
S. F. school exhibit		Realistic	
Maryland	Topographic	Conventional and realistic; cream white, blue water.	
Mass. school exhibit	Topographic	Conventional and realistic.	
	Economic geology		
New Hampshire	Topographic		
Kentucky	Geologic		
Kentucky	Forestry		
N. J. school exhibit	Topographic	Conventional and realistic.	
Ward's Nat. Sc. Est	School and political		
Mo. Geol. Survey	Geologic		
	Topographic	Brown; strong county lines.	
Brazil			
U. S. G. S.	Economic	Conventional and realistic; culture red.	
U. S. G. S.	Economic	Conventional and realistic.	Lakes covered with glass.
U. S. G. S.	Economic		
U. S. G. S.	Geologic	Water blue, geology in primary colors.	
Ward's Nat. Sc. Est			
U. S. G. S.	Geologic		Yosemite and Niagara on same model.
Ward's Nat. Sc. Est	Geologic		
Ward's Nat. Sc. Est	Geologic		
Ward's Nat. Sc. Est	Geologic	Uniform green	Stereogram.
U. S. G. S.	Geologic		
Ward's Nat. Sc. Est			
U. S G. S.	Geologic	Uniform green	Stereogram.
Ward's Nat. Sc. Est			
Ward's Nat. Sc. Est	Geologic		
U. S. G. S.	Geologic		
U. S. G. S.	Geologic	Water blue, culture white, geology primary.	
Ward's Nat. Sc. Est			
U. S. G. S.	Topographic	Culture black, water blue, land gray.	
Ward			
Ward's Nat. Sc. Est	Topographic	Green and white	
U. S. G. S.	Topographic	Culture white, water blue, land white and green.	
U. S. G. S.	Geologic	In primary colors	
	Geologic		

Relief Maps at the World's Columbian

Map.	Scales. Horizontal.	Vertical.	Ratio.	Size, inches.	Maker.
Mount Desert, Me.	1 : 40,000	1 : 40,000	1 : 1	18 x 20	E. E. Howell
Vesuvius and Monte Somma	18,000	18,000	1	28 x 28	
Crater lake, Oreg.	21,120	21,120	1	37 x 51	V. Mindeleff
Cinder cone	6,000	6,000	1	24 x 40	V. and C. Mindeleff
Cinder cone	6,000	6,000	1	24 x 40	V. and C. Mindeleff
Coon Butte, Ariz.	3,600	3,600	1	48 x 48	V. Mindeleff
Ice Spring craters, Utah	1,200	1,200	1	48 x 48	E. E. Howell
Elk mountains, Colo.	63,360	31,680	2	25 x 48	W. H. Holmes
Eureka district	19,200	19,200	1	62 x 66	E. E. Howell
Leadville and vicinity	9,600	9,600	1	32 x 38	E. E. Howell
Leadville and vicinity	9,600	9,600	1	32 x 38	E. E. Howell
Leadville and vicinity	9,600	9,600	1	32 x 38	E. E. Howell
Leadville and vicinity	9,600	9,600	1	32 x 38	E. E. Howell
Washoe mining region	20,000	20,000	1	30 x 42	E. E. Howell
Connellsville coke region	14,400	4,800	3	18 x 28	E. B. Harden
Cumberland valley				28 x 60	E. B. and J. H. Harden
Richmond sheet, Mo.	48,000	12,000	4	18 x 23	E. E. Howell
Iron Mountain sheet, Mo.	48,000	24,000	2	18 x 23	E. E. Howell
Iron mountain	1,440	960	1.5		H. D. Card
Coblenz	25,000	6,250	4		Dr. F. Vogel
Salzbrunn	25,000	6,250	4		C. Buenecke
Eisfeld	25,000	6,250	4		C. Buenecke
Jena	25,000	6,250	4		C. Buenecke
Oberharzes	125,000	6,250	20	60 x 100	Sandkuhl
Harzgebirges	100,000			24 x 36	Franz Bönecke
Same in plaster	100,000			24 x 36	K. Busz
Yosemite, Cal.	16,200	16,200	1	30 x 48	E. E. Howell
Yosemite, Cal.	16,200	16,200	1	30 x 48	E. E. Howell
Yosemite, Cal.	16,200	16,200	1	30 x 48	E. E. Howell
Yosemite, Cal.	12,000	12,000	1	48 x 72	E. E. Howell
Yosemite, Cal.	126,720	126,720	1	5 x 7	E. E. Howell
Palestine	400,000	80,000	5		Clara Deckert
Palestine	168,000			51 x 93	Armstrong
Death valley	253,440				Mindeleff
Navassa island, W. I.	3,600				Geo. M. Lehman
Saint Thomas, W. I.	380,160	126,720	3		C. E. Taylor & Son
Off-shore fishing banks between Massachusetts and New Foundland.			Large.		
Carmel bay, Cal.	12,000	12,000	1	25 x 36	E. E. Howell
Rock Creek park, D. C.	4,800	4,800	1		C. & G. S.

Exposition, Chicago, 1893—Continued.

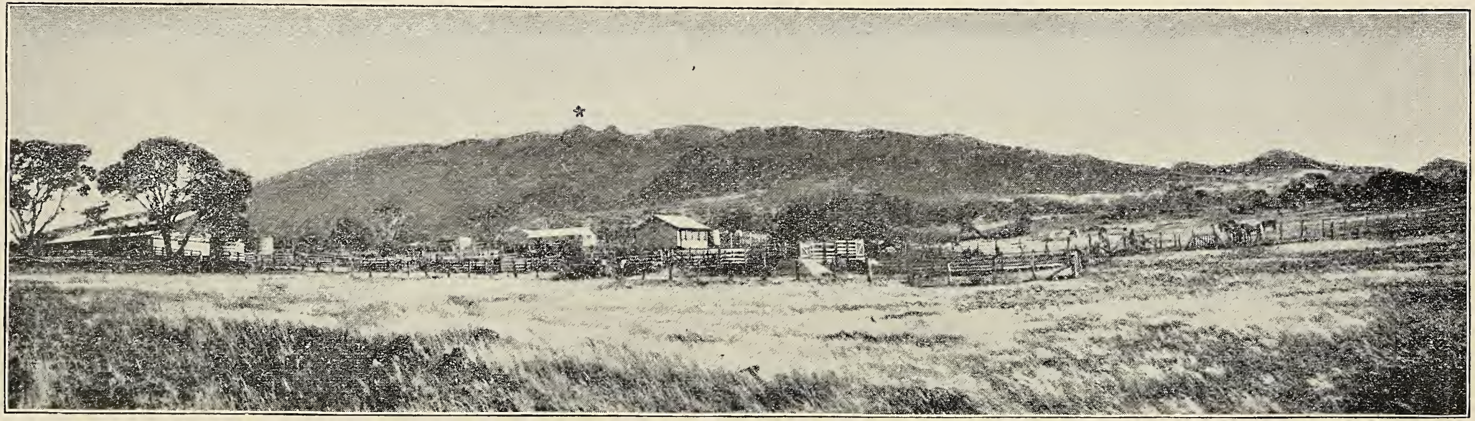
Exhibitor.	Purpose.	Color scheme.	Remarks.
C. & G. S.	Topographic	Water blue, forest green, land gray, culture white.	
Ward's Nat. Sc. Est.	Geologic	Lava flows in strong browns and yellows.	
U. S. G. S.	Topographic	Water blue, land greenish	Lake covered with glass.
U. S. G. S.	Topographic	Realistic, water blue, land lava-colored.	
U. S. G. S.	Topographic	Realistic, water blue, land lava-colored.	Dissected.
U. S. G. S.	Geologic	Realistic colors, white, yellow, red, etc.	
U. S. National Museum	Topographic	Realistic colors reddish brown and black.	
U. S. G. S.	Geologic	Geology in primary colors	Dissected.
Ward's Nat. Sc. Est.	Geologic		
Ward's Nat. Sc. Est.	Geologic	Culture black, geology in primary colors.	
U. S. G. S.	Geologic	Culture black, geology in primary colors.	
U. S. G. S.	Geologic	Culture black, geology in primary colors.	Dissected.
Ward's Nat. Sc. Est.	Geologic	Culture black, geology in primary colors.	Dissected.
Ward's Nat. Sc. Est.	Topographic	Uniform green	Not modeled; built by 50-foot contours.
H. C. Frick Coke Co.		Cream	
	Railroad map	Creamy white	
Mo. Geol. Survey	Geologic		
Mo. Geol. Survey	Geologic		
Mo. World's Fair Com.			
Germany			Not modeled; built up by contours.
Germany			Not modeled; built up by contours.
Germany			Not modeled; built up by contours.
Germany			Not modeled; built up by contours.
Germany			Not modeled; built up by contours.
Germany	Geologic		Contours only; no modeling.
Germany			
U. S. G. S.	Topographic	Realistic, water blue, land brown, culture white.	
Ward's Nat. Sc. Est.	Topographic	Realistic, water blue, land brown, culture white.	
California			
Ward's Nat. Sc. Est.	Topographic		
	Topographic	Culture white, topography brown.	Separate from Grand Canyon model.
	Topographic	Green and brown	
Pal. exploration fund	Topographic		
	Life zones		
	Topographic		
C. & G. S.	Topographic		Built up by contours.
C. & G. S.			Built up in 5-foot contours.

Relief Maps at the World's Columbian

Map.	Scales.			Size, inches.	Maker.
	Horizontal.	Vertical.	Ratio.		
	1 :	1 :	1 :		
Panuco River mouth, Mexico	2,400	120	20	36 x 60	E. E. Howell
Northeastern Iowa	250,000	36,000	6.9	38 x 40	V. and C. Mindeleff
Washington and vicinity	31,680	10,560	3	38 x 55	V. and C. Mindeleff
Baltimore and vicinity	15,840	3,600	4.4	52 x 67	C. Mindeleff
Lake George	...	...	...	...	...
Railroad from coast to Caracas.	...	...	...	...	...
Pennsylvania railroad, Altoona to Johnstown.	14,400	4,800	3	48 x 144	E. E. Howell
Pennsylvania railroad, horse-shoe curve.	...	...	...	48 x 48	V. Mindeleff
Pennsylvania railroad, bridge and tunnel.	...	...	...	48 x 48	V. Mindeleff
Nicaragua canal	...	...	15	...	E. E. Court
Canal system of New York	31,680	720	44	...	T. C. Leutze
Nord-Ostsee canal	16,666	3,300	5	54 x 300	...
New York Harbor improvement.	1,000	250	4	36 x 54	...
Hudson River improvement	5,000	1,280	4	...	...
Water supply, Havana, Cuba	250	...	...	54 x 108	Spanish engineers
Bilbao	5,000	2,500	2	60 x 132	Spanish engineers
Cartagena	5,000	2,500	2	144 x 144	Spanish engineers
San Sebastin	5,000	2,500	2	96 x 96	Spanish engineers
Fortress of Jaea	5,000	...	...	96 x 96	Spanish engineers
Battery of Cartagena	100	...	...	96 x 120	Spanish engineers
Manitou Springs, Colo	...	...	...	...	...
Trinidad asphalt lake	...	...	...	...	...
Alameda county, Cal	...	No scales	...	20 x 25 ft.	...
San Francisco, Cal	...	...	...	...	...
Zigzag railway, N. S. W	...	No scales	...	10 x 20 ft.	...
Bay of North America, etc	2,400,000	72,000	.33	48 x 60	A. and H. Lindenkohl.

Exposition, Chicago, 1893—Continued.

Exhibitor.	Purpose.	Color scheme.	Remarks.
Mexican Central R. R.	Economic	Realistic	
U. S. G. S	Topographic	White, water blue	
U. S. G. S.	Geologic	Blue water, black culture, primary colors.	
U. S. G. S.	Geologic	Blue water, black culture, primary colors.	
Venezuela			
Pennsylvania R. R.	Topographic	Realistic	
Pennsylvania R. R.	Topographic	Realistic	
Pennsylvania R. R.	Topographic	Realistic	
Nicaragua Canal Co.		Realistic, canal line red	
		Realistic	
Imp. Canal Com'n of Kiel, Germany.			
U. S. War Dep't	Economic	Realistic	
U. S. War Dep't	Economic	Realistic	
			Model blending with painted landscape.
			Actual trees, houses, etc.
			Actual trees, cars, houses, etc.
			Actual city; environs painted on surrounding wall.
			Actual bridges, miniature trees, etc.
C. & G. S.			



PRESTON. MEAN DENSITY OF THE EARTH.

VIEW OF MAUNA KEA (ELEVATION 13825 FEET) AS SEEN FROM KALAIEHA, LOOKING NORTH.

The force of gravity was determined on the summit at the point indicated by ✻

MEAN DENSITY OF THE EARTH.

BY

Erasmus Darwin Preston.

[Read before the Society, April 28, 1894.]

The mean density of the earth has been determined several times by comparing its attraction with that of a mountain. Maskelyne first applied the method to Schehallien, a mountain 3,561 feet high in Scotland. The experiment was repeated by the English ordnance survey officers in connection with their work around Edinburgh on a hill 823 feet high. The present paper is the result of an application of the same method on a much larger scale, the mountains used having altitudes of 10,000 and 14,000 feet respectively. Moreover, the Hawaiian Islands offer peculiar advantages for the solution of the problem, because the mountains rise directly from a deep sea and there is very little plain attraction to complicate the solution. Reduced to its simplest expression, the method is this: The attraction of the mountain on a plumb line is determined by connecting two points on the north and south side of it by a system of triangles. The latitudes of these two points are determined by astronomical observations, and their positions, compared with those derived by triangulation, give the amount of deviation of the plumb line. This deviation evidently depends upon the relative attractions of the mountain and the earth. Expressing the attractions in terms of the volumes, densities, and distances of the attracting masses and comparing the resulting ratios with the deviation of the plumb line, we

have an equation in which the densities of the mountain and of the earth are the only unknown quantities. The substitution of the former of these quantities given by a study of the rocks composing the mountain leads to a complete determination of the only remaining quantity—the mean density of the earth.

The first case treated is on the island of Maui. The extinct volcano Haleakala occupies practically the whole island, as far as attractions on its south flank are concerned, West Maui being too distant and too far to the west to exert much influence on the plumb line at Kaupo, in the direction of the meridian. The principal part of the island is of somewhat regular outline, about 30 miles from east to west, 25 miles from north to south, and slightly over 10,000 feet high. The triangulation connecting Haiku and Kaupo is so well expanded that it is possible to derive the geodetic position of the latter point through two separate chains of triangles which greatly strengthens the result. Moreover, the mean density of the mountain was independently determined by measuring the force of gravity near the sea level and at the summit. The rock specimens gathered by our party and those previously studied by Professor E. S. Dana are amply sufficient to give a close approximation to the mean density of the mountain, but a check was had on the result by assuming the value of 5.576 given by Harkness for the earth's mean density and deriving a value of the density of the mountain as a whole from the pendulum observations.

The determination of gravity also threw light on a question of geological interest. Various opinions were held as to the way in which Haleakala was formed. The adoption or rejection of certain theories depended entirely on whether the mountain was solid or hollow—whether it was as heavy as would appear from its volume and supposed density. The pendulum work indicated that there was no defect of gravity below the mountain, as has frequently been found beneath similar formations, and that, as a whole, its average density was approximately equal to that of the surface rocks.

It should be stated that the result of the work on Maui is due entirely to the liberality of the Hawaiian government. The triangulation had been completed before my visit and the gravity and latitude observations were made with the aid of the government survey staff, Professor W. D. Alexander, the surveyor general, being present much of the time. On Hawaii the expenses were principally borne by the Hawaiian survey and I again had the efficient services of several of the officers connected therewith. Mr. F. S. Dodge and Mr. W. A. Wall both took part in the gravity determinations on Haleakala. On Mauna Kea, besides Professor Alexander, the survey corps was represented by Messrs. W. E. Wall, E. D. Baldwin, and W. Chamberlain. Mr. J. M. Muir accompanied the party as voluntary observer. The steward was Mr. Louis Koch, and Kauwe, an experienced Kanaka guide, led the expedition from Kalaieha to the summit at Waiau.

Assuming that the law of attraction is that of the inverse square of the distance, the measure of the force f, exerted by any mass, M, at a distance, d, from the attracted point, may be written

$$f=\frac{M}{d^2}, \tag{1}$$

the unit of force being that force which acting during a unit of time will impart to a unit of mass a unit of acceleration. The above formula is to be interpreted in such units; otherwise it is necessary to introduce a constant factor, k, which will then represent in C. G. S. units the force which will give one centimeter of acceleration to a mass of one gram in one second. Then the actual force f, in dynes, will be given by the formula

$$f=k\frac{M}{d^2}, \tag{2}$$

when the symbols M and d are expressed in grams and centimeters. The value of k is 0.000 000 067 or $67 \div 10^9$. The attraction of any irregular mass may be had by finding an

expression, similar to the above in general terms, and then integrating between limits which include the mass in all three of its dimensions.

In Fig. 15, a represents an angular quantity, and r and z linear ones. If we take the elementary mass bounded by the lines $r_2\, a_2 - r_1\, a_1$, $r_2 - r_1$, and $z_2 - z_1$ (z being measured perpendicular to the plane of the paper) and if we take these differences small enough to be used as differentials, the mass

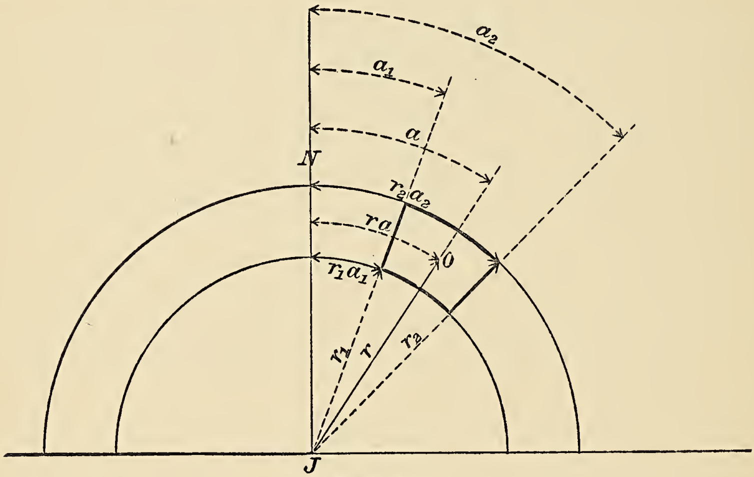


FIG. 15.

of matter occupying the elementary space above considered is

$$\delta\ r\ da\ dr\ dz, \tag{3}$$

where δ is put for the density of the matter. The attraction exerted by this matter on the point J will be the mass divided by the square of the distance and multiplied by the constant k or

$$f = \frac{k\ \delta\ r\ da\ dr\ dz}{r^2 + z^2}. \tag{4}$$

This is the effect in a line joining O and J. To reduce it to a horizontal plane passing through J, it must be dimin-

ished in the ratio of the cosine of the angle to unity or in the ratio $\frac{r}{(r^2 + z^2)^{\frac{1}{2}}}$. Multiplying these, we have the attraction at J in the line formed by the intersection of a vertical plane through $O\ J$ and a horizontal plane through J. In order now to reduce this effect to the direction $N\ J$ we must multiply it by the cosine of the angle a. Thus we obtain for the attraction of the elementary mass, O, in the direction $N\ J$ and in a horizontal plane the expression

$$f_o = \frac{k\ \delta\ r^2 \cos a\ da\ dr\ dz}{(r^2 + z^2)^{\frac{3}{2}}}. \tag{5}$$

For the total effect this expression must be integrated, between limits, for the three variables, azimuth, distance, and height, and therefore the total attraction is

$$F = k\ \delta \int_{a_1}^{a_2} \int_{r_1}^{r_2} \int_{o}^{h} \frac{r^2 \cos a\ da\ dr\ dz}{(r^2 + z^2)^{\frac{3}{2}}}. \tag{6}$$

The result of this integration is, when the vertical heights are small compared with the horizontal distances,

$$F = k\ \delta\ h\ (\sin a_2 - \sin a_1)\ \text{Nap. log}\ \frac{r_2}{r_1}. \tag{7}$$

The above integration is not difficult and is given in Clarke's Geodesy, page 295. Its explanation, however, is not given, and it is here stated in order that we may have a clear conception of its application.

From formula (7) it is evident that if the mountain be divided into rectangular prisms bounded by radial planes and concentric cylinders in such way that the sines of the azimuths of the planes are in arithmetical progression and the radii of the cylinders are in geometrical progression the attractions for compartments will vary directly as the heights of those compartments. With such division, therefore, the total attraction may be found by simply summing the heights

and multiplying by a common factor. The complete integral is *

$$F = k\,\delta\,h\,(sin\,a_2 - sin\,a_1)\,Nap.\,log\left(\frac{r_2 + \sqrt{r_2^2 + h^2}}{r_1 + \sqrt{r_1^2 + h^2}}\right). \qquad (8)$$

When the difference between the radii is taken so small that we may neglect terms containing higher powers of $r_2 - r_1$ the attraction may be written

$$F = k\,\delta\,(r_2 - r_1)\,(sin\,a_2 - sin\,a_1)\frac{h}{\sqrt{r^2 + h^2}}, \qquad (9)$$

where r is the mean value between the two radii above considered. In cases where h^2 may be neglected, we have

$$F = k\,\delta\,(r_2 - r_1)\,(sin\,a_2 - sin\,a_1)\frac{h}{r}. \qquad (10)$$

We shall show later that a sensible error would be committed by throwing out h^2 in some compartments of Haleakala.

Equations (7) and (10) offer two distinct ways of calculating the attraction. The former is the more rigorous and is approximate only to the extent of omitting the square of the heights. The accuracy of the latter depends on the relations between the horizontal distances as well as the values of h. The fact that both formulæ can be applied under certain conditions furnishes a method of checking the work. This check will be applied to the leeward side of the mountain, which gives by far the greater part of the attraction.

Let us suppose that from Kaupo (Ka Lae o Ka Ilio) a meridian and a parallel of latitude are drawn (Plate 8). On each side of the meridian draw 9 radial lines, making angles with the meridian whose sines have a common difference of $\frac{1}{10}$. Draw also 10 concentric circles, the radius of the first being one mile, and the succeeding radii increasing each by one mile over the preceding one. From the 10 mile point the radii increase in a geometrical ratio of $\frac{11}{10}$. The

* Clarke (*col.* A. R.) Geodesy. 8°. *Oxford, Clarendon press*, 1880, p. 295.


PART OF MAUI,
HAWAIIAN ISLANDS,
SHOWING
CONTOUR LINES AND COMPARTMENTS.
SCALE: 1/264,000
Miles
Pendulum
Haiku
Makawao
Olinda
Puu o Kakae
Puu Nianiau
6860 Ft.
Ana moe Haole
9287 Ft.
Koolau Gap
Crater of Haleakala
7532 Ft.
Pendulum
10032 Ft.
Pakaoao
Kaupo Gap
Upper limit of trade wind
Hana Plantation
Kauiki
Kipahulu Plantation
Kaupo
Ka Lae o Ka Ilio
16.1 Miles
25.9 Miles
20°55'
20°50'
20°45'
20°40'
20°35'
156°25'
156°20'
156°15'
156°10'
156°05'
156°00'

space inside the 10 mile limit is also to be conceived as divided by concentric circles whose radii have the same geometric relation to each other, but which are not drawn in the figure to avoid complicating the diagram, since starting with a radius of one mile and increasing in this ratio we should have the eighth circle, where for the arithmetical division we now have the second. The 10 mile limit above chosen includes the crater, the highest peak of the mountain (Kolekole) and the pendulum station at Pakaoao. The sectors count from left to right and are designated by Roman numerals. The radii are indicated by Arabic figures and increase from within outward. The compartment in sector I and between radii 1 and 2 may be written thus, I_1^2, and so on for others, so that the ring between these radii would be

$$\mathrm{I}_1^2 + \mathrm{II}_1^2 + \mathrm{III}_1^2 \ldots\ldots\ldots\ldots\ldots\ldots \mathrm{XIX}_1^2$$

The next ring would be

$$\mathrm{I}_2^3 + \mathrm{II}_2^3 + \mathrm{III}_2^3 \ldots\ldots\ldots\ldots\ldots\ldots \mathrm{XIX}_2^3$$

and so on. The compartment heights are given in the following tables, pages 376 and 377.

The attraction of compartment II_1^2 by formula (8) would be

$$F = k\,\delta \times 0.0189 \times \tfrac{1}{10} \times \Big[\mathit{Nap.\,log}\,(1.12 + \sqrt{1.12^2 + 0.0189^2}) - \mathit{Nap.\,log}\,(1.02 + \sqrt{1.02^2 + 0.0189^2}) \Big]. \qquad (11)$$

all distances being taken in miles. If we take a mean height for the entire ring between circles 1 and 2, we have 0.075 miles and the attraction for the ring would be given by using this value for the height and multiplying the result by the number of compartments. The table on page 378 gives the computation in condensed form.

Compartment Heights (Sectors I to X).

Unit = 100 Feet.

Circle.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.	Sums.
1 to 2	0.0	1.0	3.0	4.0	4.0	5.5	6.0	6.0	6.0	6.0	41.5
2 " 3	0.0	2 5	4.0	5.0	6.0	7.0	7.0	7.0	7.5	7.5	53.5
3 " 4	1.0	3.0	5.0	6.0	7.0	8.0	8.0	8.5	9.0	9.0	64.5
4 " 5	1.5	4.0	6.5	7.5	9.0	9.5	10.5	10.5	10.5	10.5	80.0
5 " 6	1.5	5.0	7.5	9.0	11.0	11.0	11.0	12.0	12.0	12.0	92.0
6 " 7	2.5	5.5	8.0	11.0	12.0	12.0	12.5	12.5	13.0	12.5	101.5
7 " 8	2.5	6.0	10.0	12.0	12.0	12.5	14.0	14.0	14.5	14.0	111.5
8 " 9	2.5	7.0	10.5	12.0	12.5	13.0	15.0	16.0	16.0	16.0	120.5
9 " 10	2.5	8.0	12.0	12.5	13.0	15.0	16.0	17.0	17.0	17.0	130.0
10 " 11	2.0	9.0	12.0	13.0	14.0	16.0	17.0	17.5	18.0	18.0	136.5
11 " 12	2.0	9.5	12.5	14.0	15.5	17.5	18.0	19.5	21.0	21.0	150.5
12 " 13	2.0	10.0	13.0	16.0	17.0	18.0	20.5	23.0	25.0	25.0	167.5
13 " 14	2.0	12.0	16.0	17.0	18.0	20.5	24.0	26.0	28.0	28.0	191.5
14 " 15	3.0	13.0	17.0	19.0	21.0	25.0	28.0	32.5	33.0	33.0	224.5
15 " 16	6.0	17.0	19.0	23.0	27.0	31.0	35.0	40.0	40.0	38.0	276.0
16 " 17	6.0	18.0	23.0	27.0	33.0	40.0	45.0	50.0	44.0	42.0	328.0
17 " 18	11.0	23.0	28.0	34.0	40.0	50.0	55.0	57.0	47.0	46.0	391.0
18 " 19	14.0	28.0	34.0	41.0	55.0	60.0	65.0	64.0	50.0	49.0	460.0
19 " 20	16.0	34.0	40.5	55.0	65.0	72.0	80.0	70.0	57.5	55.0	545.0
20 " 21	21.0	39.0	52.0	65.0	76.0	81.0	75.0	70.0	65.0	65.0	609.0
21 " 22	25.0	46.5	64.0	80.0	77.0	75.0	75.0	75.0	70.0	70.0	657.5
22 " 23	30.0	54.0	74.0	80.0	75.0	75.0	75.0	76.0	80.0	80.0	699.0
23 " 24	35.0	66.0	90.0	74.0	75.0	75.0	75.0	83.0	70.0	69.0	712.0
24 " 25	40.0	74.0	100.0	77.0	75.0	75.0	75.0	70.0	65.0	62.0	713.0
25 " 26	44.5	79.5	83.5	87.5	80.5	71.5	64.0	62.0	60.0	54.0	687.0
26 " 27	56.0	73.0	77.5	77.5	76.5	69.5	58.5	54.5	52.5	45.5	641.0
27 " 28	42.5	64.0	65.0	65.5	63.0	59.5	54.5	46.0	41.0	36.0	537.0
28 " 29	43.0	49.0	49.0	51.5	51.0	50.0	47.0	39.0	29.0	22.5	431.0
29 " 30	34.0	32.5	34.0	38.5	41.0	42.0	40.0	32.5	19.0	9.5	323.0
30 " 31	15.5	19.5	22.5	28.0	30.5	35.5	29.0	22.0	10.0	......	212.5
31 " 32	8.0	11.5	15.0	19.5	22.0	22.0	19.0	12.0	......	......	129.0
32 " 33	....	6.5	9.5	13.0	15.5	15.0	10.5	9.0	......	......	79.0
33 " 34	....	3.0	5.5	7.5	9.0	8.5	5.5	......	......	......	39.0
34 " 35	....	1.0	3.0	3.0	3.0	1.0	......	......	......	......	11.0

Compartment Heights (Sectors XI to XIX).

Unit = 100 Feet.

Circle.	XI.	XII.	XIII.	XIV.	XV.	XVI.	XVII.	XVIII.	XIX.	Sums.
1 to 2	6.0	5.0	5.0	4.0	4.0	3.0	3.0	2.5	1.0	33.5
2 " 3	7.0	6.0	7.0	5.5	5.0	4.0	3.5	2.5	1.0	41.5
3 " 4	9.0	8.0	8.0	7.0	6.0	4.5	4.0	3.0	1.0	50.5
4 " 5	11.0	10.0	10.0	9.0	7.5	6.0	4.5	3.0	2.0	63.0
5 " 6	12.0	11.0	11.0	10.5	9.0	7.0	5.0	3.0	2.0	70.5
6 " 7	12.5	12.0	12.0	11.0	10.0	8.0	5.0	3.0	2.0	75.5
7 " 8	14.0	12.5	12.5	12.0	11.0	9.0	5.0	3.0	2.0	81.0
8 " 9	15.5	14.0	14.5	13.0	12.0	10.0	6.0	4.0	2.0	91.0
9 " 10	17.0	15.5	15.5	14.0	12.5	11.0	7.0	4.0	2.0	98.5
10 " 11	18.0	17.5	17.0	14.5	13.0	12.5	10.0	5.0	1.0	108.5
11 " 12	21.5	19.5	18.0	15.5	15.0	14.5	12.0	6.0	0.5	122.5
12 " 13	24.5	22.0	20.0	17.0	19.0	17.5	14.0	8.0	2.0	144.0
13 " 14	27.0	26.0	24.0	21.0	22.0	21.0	16.0	10.0	2.5	192.0
14 " 15	32.0	31.0	30.0	24.0	25.0	23.0	19.0	11.0	3.0	198.0
15 " 16	37.0	37.0	34.0	30.0	32.0	27.0	22.0	15.0	5.0	239.0
16 " 17	40.0	43.0	40.0	36.0	34.0	30.0	23.0	16.0	7.0	269.0
17 " 18	47.0	54.0	50.0	41.0	39.0	33.0	27.0	17.0	8.0	316.0
18 " 19	55.0	60.0	56.0	50.0	44.0	35.0	27.0	20.0	10.0	357.0
19 " 20	65.0	67.0	62.0	55.0	45.0	35.0	25.0	21.0	10.0	385.0
20 " 21	75.0	60.0	50.5	45.0	42.0	37.0	28.0	20.0	10.0	367.5
21 " 22	72.0	65.0	55.0	47.0	40.0	37.0	30.0	26.0	9.0	381.0
22 " 23	76.0	69.0	62.0	56.0	47.0	40.0	40.0	27.0	7.0	424.0
23 " 24	65.0	59.0	55.0	52.0	46.0	40.0	40.0	32.0	5.0	394.0
24 " 25	55.0	50.0	46.0	45.0	45.0	42.0	40.0	32.0	6.0	361.0
25 " 26	47.0	39.5	36.0	35.5	37.5	37.5	34.5	22.5	1.0	291.0
26 " 27	37.5	30.0	26.5	28.0	29.0	31.0	28.5	16.0	0.0	226.5
27 " 28	28.5	21.0	18.5	19.5	21.5	20.0	14.5	5.5	....	149.0
28 " 29	16.0	7.5	7.5	7.0	5.0	6.5	4.5	1.0	....	55.0
29 " 30	3.5									
30 " 31										
31 " 32										
32 " 33										
33 " 34										
34 " 35										

No. of circle.	Radius.	Mean height of ring.	Nap. log. factor.	No. of Compartments.	$\frac{F}{k\delta}$.
	Miles.	*Miles.*			
1	1.02	0.075	0.09255	19	0.0132
2	1.12	0.095	.09360	19	.0169
3	1.23	0.115	.09298	19	.0203
4	1.35	0.143	.09880	19	.0268
5	1.49	0.161	.09534	19	.0292
6	1.64	0.176	.09285	19	.0310
7	1.80	0.191	.09481	19	.0344
8	1.98	0.210	.09574	19	.0382
9	2.18	0.227	.09591	19	.0414
10	2.40	0.244	.09459	19	.0438
11	2.64	0.272	.09352	19	.0483
12	2.90	0.310	.09489	19	.0559
13	3.19	0.382	.09498	19	.0690
14	3.51	0.421	.09447	19	.0756
15	3.86	0.513	.09550	19	.0931
16	4.25	0.595	.09349	19	.1056
17	4.67	0.705	.09489	19	.1271
18	5.14	0.814	.09361	19	.1448
19	5.65	0.927	.09335	19	.1644
20	6.21	0.972	.09418	19	.1739
21	6.83	1.035	.09393	19	.1847
22	7.51	1.120	.09427	19	.2006
23	8.26	1.103	.09496	19	.1990
24	9.09	1.071	.09480	19	.1929
25	10.00	0.975	.09492	19	.1758
26	11.00	0.864	.09504	18	.1478
27	12.10	0.684	.09520	18	.1172
28	13.31	0.484	.09518	17	.0783
29	14.64	0.325	.09502	11	.0340
30	16.10	0.211	.09531	9½	.0191
31	17.71	0.129	.09523	8½	.0104
32	19.48	0.079	.09540	8	.0060
33	21.43	0.039	.09518	6	.0022
34	23.57	0.011	.09543	5	.0005
35	25.93				

The first column has the number of the circle; the second gives the length of the corresponding radius. Following this is the mean height of the ring between the successive radii. The fourth column contains the factor given by the expression

$$Nap.\ log\ \left(\frac{r_2 + \sqrt{r_2^2 + h^2}}{r_1 + \sqrt{r_1^2 + h^2}}\right).$$

The next column gives the number of compartments in

each ring counted from the parallel of latitude through Kaupo on the left around toward the right until the shore-line is reached. Some of these are fractional compartments, but where they fall short or overlap a compensation is made in estimating the number. The last column shows the numerical part of equation (8) as far as it depends on the length of the radii, the heights of the land and the azimuths of the angles forming the sides of the compartments. The sum of this column multiplied by $k\,\delta$ gives the total attraction of the mountain in the direction of the meridian at Kaupo; so that finally we have

$$F = k\,\delta \times 2.723. \qquad (12)$$

If we take into account West Maui by the application of formula (7) we have for the

$$\text{Leeward side....}\ k\,\delta \times 0.473 \times \tfrac{1}{10} \times Nap.\,log\left(\frac{39.5}{29.3}\right);$$

$$\text{Windward side..}\ k\,\delta \times 0.284 \times \tfrac{1}{10} \times Nap.\,log\left(\frac{40.8}{33.2}\right);$$

giving a total attraction of $0.020 \times k\,\delta$. So that the entire influence of the island of Maui on the plumb-line at Kaupo in the direction of the meridian is

$$F = k\,\delta \times 2.743. \qquad (13)$$

The attraction of the earth on a point at its surface is $\frac{4}{3}\,k\,\pi\,\Delta\,R$, where Δ is the mean density, R the radius, and k, as before, is the constant of gravitation. Assuming R to be 3,960 miles, the earth's attraction is $16{,}588\,k\,\Delta$. The angular deflection of the plumb line at Kaupo will be equal to the attraction of the mountain divided by the attraction of the earth, and we shall have

$$D = \frac{2.743}{16588}\,\frac{\delta}{\Delta} = 2.743\,\frac{\delta}{\Delta} \times 12''.44 = 34''.12\,\frac{\delta}{\Delta}. \qquad (14)$$

The preceding equation takes account of the matter lying above the sea-level. The effect of the sea-water would be to increase the deflection of the plumb-line at Kaupo, inasmuch as the space below the level of the sea is filled with matter which is heavier on the north side than on the south. The influence of this matter would be that of a mountain having a density equal to the difference of the densities of the land and sea, or

$$\frac{2.90-1}{5.576}=\frac{1.90}{5.58},$$

the value of 5.576 being taken from Harkness * and the value 2.90 results both from a study of the surface rocks and from a determination of the density of the mountain as a whole by means of the pendulum. If in equation (14) we should use $\delta-\frac{\Delta}{5.58}$ instead of δ, we would have

$$D=34''.12\frac{\delta}{\Delta}\left(1-\frac{1}{\delta}\right);$$

so that the attraction calculated for the matter above the sea-level must be multiplied by $\frac{\delta-1}{\delta}$, or 0.655, to get the effect of the sea water, and the combined effect of land and sea would be

$$D=34''.12\frac{\delta}{\Delta}\times 1.655=56''.47\frac{\delta}{\Delta}.$$

If we consider the ring between the 24th and 25th circles and calculate the attraction for the highest compartment, assuming 1.5 miles for h, we get for the result 0.01397 δ, whereas by neglecting h^2 we have 0.01430 δ, giving a difference of about one part in forty. As a matter of fact, there are many compartments where h is even greater and where the distance to the attracted point is still less, both of which circumstances would augment the attraction. It has therefore been deemed advisable to consider the second power of the height in all compartments, although it has doubtless

* Harkness (Prof. Wm.) The solar parallax and its related constants. 4°. Washington, 1891. [Appendix III. Washington observations, 1885.]

been used in a few rings where its omission would have been immaterial. The space between Kaupo and the 25th circle was also considered in circles with radii in an arithmetical progression giving 10 rings. The attraction was calculated for these by formula (10). The result for the attraction agreed satisfactorily with that from the regular geometric division and showed that no sensible error had occurred in the computations.

It may be worth while to add a word concerning the land lying to the southwest of Kaupo. This has not been considered in the computation, both on account of its position and insignificant volume. If we apply equation (7) to this matter, it is easily shown that its effect on the resulting mean density of the earth may well be neglected. In the first place, its center of gravity lies so nearly in the same latitude as Kaupo that the component of attraction in the direction of the meridian is probably not one tenth of the total attraction of this mass; besides, the mass itself is comparatively small, having on the average an elevation of about 500 feet. In fact, if we had this height of matter spread over a space equal to sector I between the 10th and 20th circles, which would augment the attraction on account of the increased difference in latitude, even then we should only have approximately

$$k\,\delta \times \tfrac{1}{10} \times \tfrac{1}{10} \times \mathit{Nap.\ log}\ 2 \text{ or } k\,\delta \times 0.007,$$

which would be of no importance in the final result. This disposes of all the matter lying above the sea level.

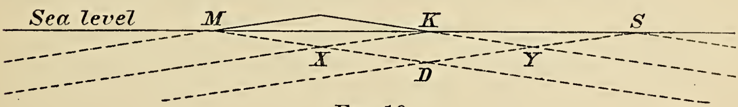


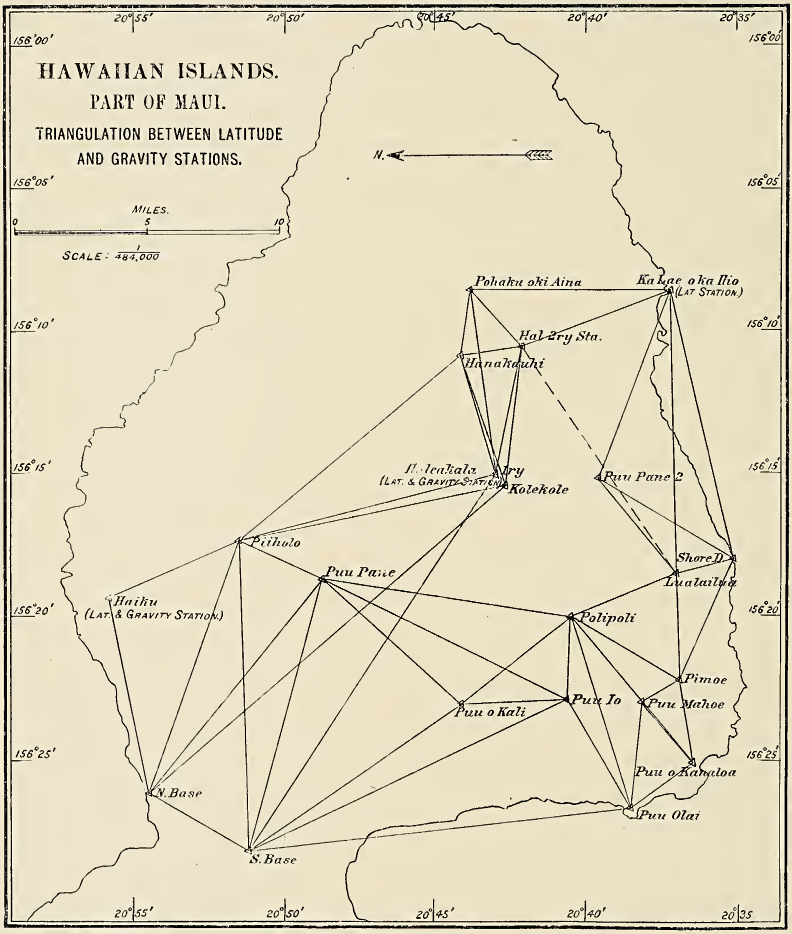
Fig. 16.

The influence of the matter below the sea level is perhaps best understood by reference to Fig. 16.

The latitude station is at K. We assume that the slope of the mountain is continuous from the summit to the bottom

of the sea, which is about as far below the surface as the top of Mauna Kea is above it. This view is entertained by Major Dutton and is borne out by soundings made around the island. The plumb line at *K* is principally influenced by the matter in the wedge, whose cross-section is *M S D*. Of this volume the effect of the parallopiped, whose cross-section is *K X D Y*, is neutral, since the matter is presumably uniform throughout and is equally divided north and south of the station. There remains, then, the two wedges, *M X K* and *K Y S*, each equal to the mountain, and the combined effect of these would be approximately that of a mountain whose density is equal to the difference of the densities of rock and sea water.

It should be borne in mind that the relative dimensions of the mountain are considerably exaggerated in the diagram. The base is about fifteen times the altitude, so that the neglected matter is in reality much further from *K* than it appears to be in the figure. If it were possible to treat the problem in the most rigorous manner, by using more exact data, the result would probably undergo some modification. For instance, the matter beyond *M* and that not included in the figures already considered doubtless do both influence the plumb line at Kaupo to a certain extent; but when we remember that we are in total ignorance as to the exact density of this matter, and that with any plausible density its effect would be a very small fraction of that already considered, the conclusion is that the use of our data has perhaps been carried far enough. Moreover, there is considerable conjecture about the slope of the mountain below the sea-level, and it would seem hardly worth while to pursue further the evaluation of the effect of these volumes with our present knowledge of the subject. A better survey of the ocean bottom around the Hawaiian Islands is very much to be desired. With more trustworthy data in this direction, this group offers exceptional advantages for the study of geological and geodetic problems of the greatest importance.



PRESTON. MEAN DENSITY OF THE EARTH.

Triangulation from Haiku to Kaupo.

The geodetic connection between the two latitude stations was made by two series of triangles. The first passes over the summit, the second is carried around the west flank of the mountain and reaches Ka Lae o Ka Ilio (Kaupo) through a chain of 15 triangles. The first series is more direct, containing only 7 triangles. The agreement between the two independently calculated positions of Kaupo leaves no doubt as to the accuracy of its position.

Plate 9 shows the location of the trigonometrical points.

The following table gives the angles, distances, and latitudes:

Geodetic connection between Haiku and Kaupo.

First Series of Triangles.

	Station.	Angle.			Distance in meters; logs.	To—	Latitude of stations in column 2.		
		°	′	″			°	′	″
1	North Base*	100	25	07.5	3.8239822	South Base*	20	54	20.0
	South Base	59	25	23	4.2794300	Piiholo	20	51	7.8
	Piiholo	20	09	29.5	4.2216265	North Base	20	51	39.8
2	North Base	29	17	39	4.2216265	Piiholo.			
	Piiholo	45	11	15	3.9273226	Haiku.			
	Haiku	105	31	06	4.0886547	North Base	20	55	43.6
3	South Base	34	16	21	4.2794294	Piiholo.			
	Piiholo	103	41	15	4.2041892	White Hill.			
	White Hill	42	02	24	3.4410687	South Base	20	43	21.15
4	Piiholo	25	55	32	4.2041892	White Hill.			
	White Hill	88	49	20	3.8867102	Hanakauhi.			
	Hanakauhi	65	15	08	4.2459353	Piiholo	20	44	37.6
5	White Hill	27	22	45	3.8867102	Hanakauhi.			
	Hanakauhi	82	02	20	3.574786	Haleakala, 2.			
	Haleakala, 2	70	34	55	3.907938	White Hill	20	42	37.2
6	White Hill, 2	18	58	00	3.907938	Haleakala, 2.			
	Haleakala, 2	126	00	45	3.661028	Palaha.			
	Palaha	35	01	15	4.057010	White Hill	20	44	21.4
7	Haleakala, 2	113	40	00	3.661028	Palaha.			
	Palaha	46	33	30	4.093537	Ka Lae-o-ka-Ilio			
	Ka Lae-o-ka-Ilio (Kaupo.)	19	46	30	3.992672	Haleakala	20	37	38.1

* Azimuth, North Base to South Base, S. 27° 35′ 25.″0, W.

Geodetic connection between Haiku and Kaupo.

Second Series of Triangles.

	Station.	Angle.			Distance in meters; logs.	To—	Latitude of stations in column 2.		
		°	′	″			°	′	″
1	North Base......	80	37	23.5	3.8239821	South Base......	20	54	20.0
	South Base.......	76	55	06.5	4.2360637	Puu Pane.......	20	51	7.8
	Puu Pane........	22	27	30	4.2304866	North Base.....	20	48	47.3
2	North Base......	19	47	43.5	4.2304866	Puu Pane.			
	Puu Pane........	76	45	24	3.7630998	Piiholo.			
	Piiholo..........	83	26	52.5	4.2216265	North Base......	20	51	39.8
3	North Base......	29	17	39	4.2216265	Piiholo.			
	Piiholo..........	45	11	15	3.9273226	Haiku.			
	Haiku...........	105	31	06	4.0886547	North Base......	20	55	43.6
4	South Base.......	38	36	50	4.2360637	Puu Pane.			
	Puu Pane	62	39	17	4.0397505	Puu-o-Kali.			
	Puu-o-Kali.......	78	43	53	4.1930553	South Base......	20	44	22.1
5	South Base.......	48	18	22	4.2360637	Puu Pane.			
	Puu Pane........	79	05	02	4.2091099	Puu Io.			
	Puu Io...........	52	36	36	4.3280282	South Base......	20	40	52.1
6	Puu Pane........	16	25	45	4.2091099	Puu Io.			
	Puu Io...........	28	38	58	3.8105550	Puu-o-Kali.			
	Puu-o-Kali.......	134	55	17	4.0397726	Puu Pane.			
7	Puu-o-Kali.......	36	13	16	3.8105550	Puu Io.			
	Puu Io...........	94	35	50	3.7030981	Polipoli.			
	Polipoli	49	10	54	3.9301788	Puu-o-Kali......	20	40	48.1
8	South Base.......	20	01	07	4.3280282	Puu Io.			
	Puu Io...........	93	02	20	3.8986264	Puu Olai.			
	Puu Olai.........	66	56	33	4.3635763	South Base......	20	38	42.6
9	Puu Io.	148	24	12	3.8986264	Puu Olai.			
	Puu Olai.........	12	12	51	4.0969330	Polipoli.			
	Polipoli..........	19	22	57	3.7031012	Puu Io.			
10	Puu Olai.........	22	12	10	4.0969330	Polipoli.			
	Polipoli..........	21	58	00	3.8311961	Puu Mahoe.			
	Puu Mahoe......	135	49	50	3.8267852	Puu Olai........	20	38	26.6
11	Polipoli	20	34	09	3.8311961	Puu Mahoe.			
	Puu Mahoe......	99	32	42	3.4398912	Pimoe.			
	Pimoe.	59	53	09	3.8881117	Polipoli.........	20	37	9.3
12	Polipoli..........	53	07	50	3.8881117	Pimoe.			
	Pimoe...........	57	58	50	3.8213768	Lualailua.			
	Lualailua........	68	53	20	3.8466126	Polipoli.........	20	37	18.8

Geodetic connection between Haiku and Kaupo.

Second Series of Triangles—Continued.

	Station.	Angle.	Distance in meters; logs.	To—	Latitude of stations in column 2.
		° ′ ″			° ′ ″
13	Pimoe...........	24 04 45	3.8213768	Lualailua.	
	Lualailua........	103 30 00	3.532937	Shore D.	
	Shore D..........	52 25 15	3.910110	Pimoe..........	20 35 32.2
14	Lualailua........	112 30 00	3.532937	Shore D.	
	Shore D..........	48 12 20	3.979482	Puu Pane, 2	
	Puu Pane, 2......	19 17 40	3.886338	Lualailua........	20 39 54.7
15	Shore D..........	44 36 35	3.979482	Puu Pane, 2.	
	Puu Pane, 2......	102 05 00	4.086318	Ka Lae-o-ka-Ilio.	
	Ka Lae-o-ka-Ilio.. (Kaupo.)	33 18 25	4.230081	Shore D.........	20 37 38.1

White Hill is the same as Haleakala Iry and is also called Pakaoao. Palaha is the "Pohaku-oki-aina," *i. e.*, the rock where the boundaries of eight districts meet. Geodetic latitude of Ka Lae o ka Ilio; 1st series, 20° 37′ 38.06″; 2d series, 38.07″.

Determination of Latitudes.

The astronomical latitudes were found by the method of equal zenith distances, a combination instrument being used for the purpose. The results for the three stations from separate pairs of stars were as follows:

	″	″	″	″	″	″	″
Haiku......	2.91	2.03	2.21	2.72	2.26	2.95	2.24
20° 56′	3.38	2.55	3.99	2.39	2.65	3.98	1.41
	3.19	2.32	4.38	2.02	2.98	1.46	3.40
	1.59	1.96	2.66	1.42	4.14	2.29	1.14
Pakaoao....	52.68	52.30	49.86	51.42	50.91	50.62	51.03
20° 42′	49.42	50.80	51.85	51.44	51.48	50.05	50.47
	50.57	50.63	50.90	51.64	50.09	52.00	51.72
	....	51.47	51.91	52.21	49.15	49.20	50.93
Kaupo......	40.77	42.24	40.77	42.15	40.14	39.59	41.65
20° 36′	40.05	39.28	42.36	41.04	40.75	42.05	41.44
	40.98	40.01	40.78	39.51	41.38	43.03	39.95
	39.11	39.26	41.60	41.92	39.58	41.70	39.56
	40.85	39.78	41.63	39.95	39.25	40.12	39.63

The final results were:

Station.	Latitude.	No. of observations.	No. of pairs.	No. of nights.
	° ′ ″ ″			
Haiku	20 56 2.57 ± 0.12	75	28	5
Pakaoao	20 42 50.99 ± 0.11	62	27	5
Kaupo	20 36 40.75 ± 0.12	90	35	6

We therefore have the following data for the determination of the deflection of the plumb line:

Summary.

Station.	Latitude.					
	Geodetic.			Astronomic.		
	°	′	″	°	′	″
Haiku:						
Trigonometric station	20	55	43.6			
Observatory	20	56	4.0	20	56	2.6
Kaupo:						
Trigonometric station	20	37	38.1			
Observatory	20	37	41.0	20	36	40.8
Difference		18	23.0		19	21.8
Deflection of plumb line					0	29.4
Pakaoao:						
Trigonometric station	20	43	21.2			
Observatory	20	43	21.6	20	42	51.0

The geodetic amplitude between Haiku and Kaupo is smaller than the astronomic amplitude by 58″.8. Taking as a standard the latitude of the base-line on Maui, a deflection of the plumb-line toward the north appears at all three stations, being 1″.4 at the most northern one, increasing to 29″.4 at Pakoao, on the summit of the mountain, and rising to nearly a minute of arc at the southern station. These are, however, merely relative values, and we cannot assume that the actual deflections are all in this direction or are ex-

actly of this amount. The astronomic latitude determined at the base-line is of course influenced by the mountain, and the reference to this station simply gives a convenient means of comparison without implying any particular relation between the individual attractions as a consequence thereof. In order to estimate how the discrepancy between the two amplitudes shall be divided between the two sides of the mountain and what part of it shall be attributed to each station we must consider the position of the center of gravity of the mass. The deflection will depend on its distance and azimuth from the points in question. To find the center of gravity a horizontal projection of Haleakala was divided into small squares and these were loaded by weights proportional to their respective heights. The position sought was then found experimentally to be near the line between sectors IV and V and not far from the 26th circle (11th in Plate 8). The azimuth from Haiku is 24° and from Kaupo 38°. Considering the mass of the mountain concentrated at this point, which we may do in view of the approximate nature of our knowledge as to its uniform density, we find that the difference in the azimuths nearly compensates for the difference in distances. At equal distances these azimuths would require about 32″ of the deflection to be at Haiku and 27″ at Kaupo, whereas with equal azimuths the difference in distance according to the law of inverse squares would demand about 26″ at Haiku and 32″ at Kaupo. The distances were taken from a large scale map ($\frac{1}{60000}$) and are probably near the truth; so that with this nearly complete compensation before us we accept the value of 29″.4 given by the triangulation and derive Δ from the equation

$$\Delta \times 29''.4 = 56''.5 \times \delta.$$

The value of δ is found to be 2.90 in the latter part of the paper, and we therefore have

Mean Density of Earth from Haleakala = 5.57.

Determination of the Mean Density of the Earth from Pendulum Observations on Mauna Kea.

The method used involved a study of the rocks composing the mountain and a determination of the decrease of the force of gravity in passing from the sea level to the summit. The formula employed is based on the assumption that the shape of the mountain is intermediate between that of a truncated cone and an extended plain. This estimate results from a comparison of photographs taken on different sides of the mountain. The gravity observations were made with Dr. Mendenhall's new half second pendulums, which attain an accuracy in the periods of oscillation of one part in two millions. The density of the mountain depends on a study of more than 40 rock specimens.

The formula usually employed in the treatment of the change of the force of gravity with elevation is:

$$\frac{dg}{g} = -\frac{2h}{r}\left(1 - \frac{3}{4}\frac{\delta}{\Delta}\right), \tag{15}$$

where g = the force of gravity at the sea level,
h = the elevation,
r = the radius of the earth,
δ = the density of the mountain, and
Δ = the mean density of the earth.

This formula is derived by combining the earth attraction, varying inversely as the square of the distance, with the attraction of the matter lying between the sea level and the upper station. Certain suppositions are made in regard to the form of this exterior matter, and it has been generally considered a sufficient approximation to regard all the matter as equivalent to that contained in a plain of infinite extent and of a thickness equal to the elevation (h). Whether this view be admissible depends, of course, on the relation between the vertical and horizontal dimensions of the intervening mass. If we suppose this to be of a conical form,

the above term, depending on the attraction of the plain, must receive the correction:

$$+\frac{3}{4}\frac{\delta}{\varDelta}\frac{h}{\sqrt{h^2+a^2}},$$

where a is the radius of the base. This expression reduces to zero for an infinite value of a in which we have the previous case of an infinite plain. For a value of a equal to zero the entire effect of the intervening matter disappears, as it should do, from the formula. The expression for the differential of gravity at the summit of a conical mountain would therefore be:

$$dg = -\frac{2\,h}{r}\,g\left(1-\frac{3}{4}\frac{\delta}{\varDelta}+\frac{3}{4}\frac{\delta}{\varDelta}\cos\beta\right), \qquad (16)$$

where β is the semi-vertical angle of the mountain. When a is very large compared with h the correction for a paraboloid is two-thirds and that for a sphere is one-half that given above.*

We have a greater value of gravity in the case of the cylinder than for the paraboloid and a greater one for the paraboloid than for the cone. In Fig. 17, gravity at Q

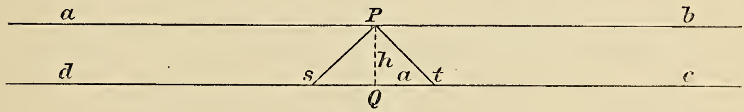


FIG. 17.

would be diminished by passing to P, if no intervening matter existed, by

$$\frac{2\,h}{r}\,g = \text{correction for distance (negative).}$$

If we interpose the infinite plain $a\ b\ c\ d$ gravity will be increased by

$$\frac{2\,h}{r}\,g\,\frac{3}{4}\,\frac{\delta}{\varDelta} = \text{correction for plain (positive).}$$

* Helmert (*Dr.* F. R.) Die math. und phys. theorieen der höheren geodäsie. 8°. Leipzig, 1884. Part 2, p. 172.

Since δ is always less than $\varDelta$, the combined effect of these two terms will be essentially negative. The difference between the effect of the plain and the cone is seen in the last term, and the total influence exerted by matter equal in volume and position to that generated by the revolution of the trapezoid $a\ P\ s\ d$ around $P\ Q$ would be

$$-\frac{2\,h}{r}\,g\,\frac{3}{4}\,\frac{\delta}{\varDelta}\,\frac{h}{\sqrt{h^2+a^2}}.$$

Since a is always very much greater than h, the expression

$$\frac{h}{\sqrt{h^2+a^2}}$$

is a small fraction and the combined effect of the last two terms will be of the same sign as the first of them—that is, their effect will be essentially positive. This is as it should be, and shows that the effect of this matter is to increase gravity at P, whereas the effect of the first two terms would necessarily be to diminish gravity at the same point, except in the extraordinary case where the infinite plain should be one-third as heavy again as the average earth matter. A uniform density of 7.56 in an infinite plain immediately under the station would exactly counterbalance the diminution of gravity on account of distance. The negative sign attributed to the last term must not be interpreted as meaning that the actual effect of this matter is to diminish the value of gravity at P. It rather means that the general effect of the plain being to increase gravity at P, the influence of $a\ P\ s\ d$ comes in here as expressing the difference between the effect of the plain and cone, and that the plain effect must be diminished by

$$\frac{3}{4}\,\frac{\delta}{\varDelta}\,\frac{h}{\sqrt{h^2+a^2}}$$

in order to get the cone effect. In fact, the parenthetical

part of the actual plain effect being $+\frac{3}{4}\frac{\delta}{\Delta}$ and that of the volume $a\ P\ s\ d$ being

$$\frac{3}{4}\frac{\delta}{\Delta}\frac{h}{\sqrt{h^2+a^2}},$$

their difference gives the actual effect for the cone as expressed in (16). For a mountain 2.5 miles high and having a radius of base equal to 30 miles, $\frac{h}{\sqrt{h^2+a^2}}=\frac{1}{12}$ nearly, so that it makes an essential difference whether we treat the mountain as a cone or as a plain.

The formula

$$\frac{dg}{g}=-\frac{2h}{r}\left(1-\frac{3}{4}\frac{\delta}{\Delta}\right)$$

has long been known as Young's rule, although it first appeared in 1749, in Bouguer's work, "La Figure de la Terre." He made the assumption that the surface density of the earth is equal to one-half the mean density—*i. e.*, $\Delta = 2\ \delta$, from which the total diminution of gravity on account of distance and matter would be $\frac{5}{4}\frac{h}{r}$. This correction has continually been applied in the treatment of mountains and table lands by most modern observers. Whether this formula is the proper one to use or whether, indeed, any correction at all should be made for continental attraction is still an open question. It has been assumed that the whole cone of matter $P\ s\ t$ might be brought down by compression to the line $d\ c$ without materially altering the shape of the sea-level; that is to say, that the vertical attraction at the point P before compression is approximately equal to that after compression. In other words, the intervening matter has no effect. This assumption has evidently been made in view of the earlier measurements of the force of gravity, which seemed to show a very small density for mountains. Two notable cases of this are the Andes, which appear to be not much heavier than ice,* and the island of Ascension, where the observed force of gravity at the sea-level was actually

* Bouguer (Pierre). La figure de la terre. 4°. *Paris*, 1749, p. 362.

more than that at the summit of Green Mountain reduced for elevation, indicating that the downward attraction of all matter above the sea level was insignificant.* In order to determine the density of Mauna Kea, an extensive collection of rocks, was made at different stations, beginning at Kawaihae on the leeward side, passing to the summit, and finally ending at Hilo, on the windward side. In all, 26 specimens were obtained. They have been carefully studied by Professor G. P. Merrill, Curator of Lithology at the Smithsonian Institution, who has kindly furnished the following densities. The points from which they were obtained are indicated in Plate 10 by numbers and letters.

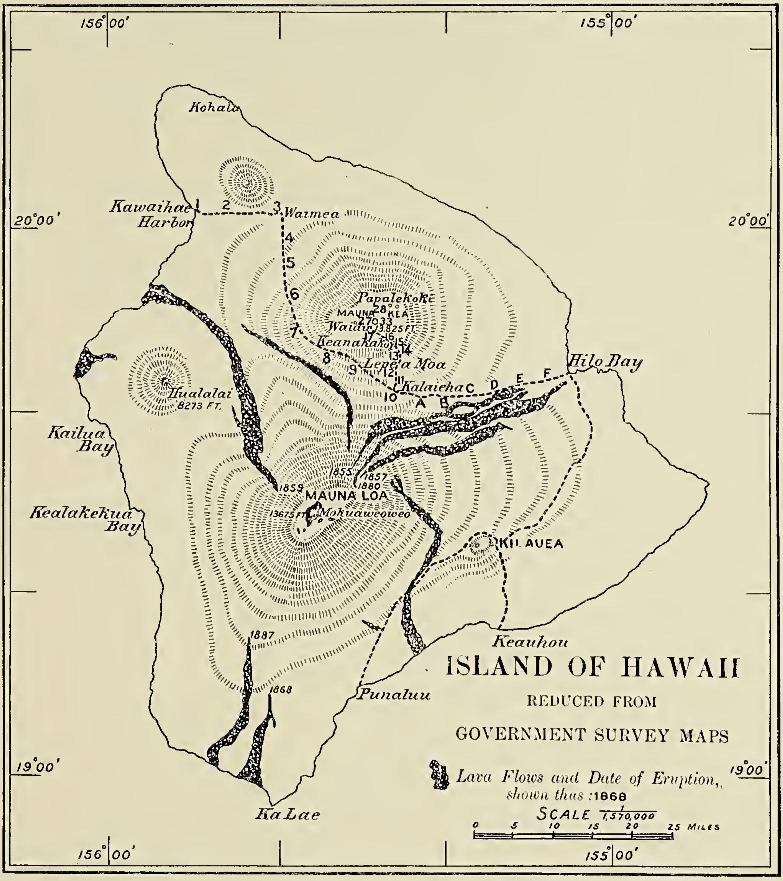
Only 18 determinations were made, as this number seemed to include all the distinct types in the collection.

Density of Rocks on Mauna Kea.

No.	Density.	Remarks.	No.	Density.	Remarks.
1	2.82		13........	2.00	
2	3.08		14........	2.73	
4	2.90		16........	3.02	In powder.
5	2.90		16........	2.99	In bulk.
9	2.79		17........	2.73	
10	2.76		18........	2.83	
11	2.84		D	2.88	
13	2.27	In powder.	23........	2.80	
13	2.12	In bulk.	23........	1.70	Scarce.

The mean of the above values is 2.68. This, according to Professor Merrill, may be slightly under the true value. A specimen from the Island of Hawaii not included in the above collection gave a value of 3.20. In Professor E. S. Dana's "Contributions to the Petrography of the Sandwich Islands" in the American Journal of Science, June, 1889, the results of a number of determinations of specific gravity are given from the same island. Seven specimens of basalt gave values ranging from 2.82 to 3.00 (p. 442). Another group of seven determinations furnished examples of even heavier lava, ranging from 3.00 to 3.20 (p. 447). If we take the

* Royal Astronomical Society. Memoirs. 4°. *London*, 1834, vol. 7, p. 60.



PRESTON. MEAN DENSITY OF THE EARTH.

mean of the preceding values we get a specific gravity of 2.90 for the specimens on the Island of Hawaii.

Mauna Kea has the form of a truncated cone, so that its effect on the force of gravity would be intermediate between that of a cone and an infinite plain. As the difference between the ratios of $\frac{\delta}{\Delta}$ for the two forms is only about $\frac{1}{12}$ of the value of either, a direct mean between them will in all probability approach closely to the actual effect of the mountain. The attraction of an infinite plain on a point above it is entirely independent of the distance of the point from the plain. In this case where the point is in the upper surface of the plain and h is its thickness, we have for the plain effect the expression

$$2 \pi \delta h \text{ or } 15.54 \delta.$$

The attraction of the earth at the upper station (Waiau) and the lower station (Kawaihae) are respectively

$$16566.8 \Delta \text{ and } 16587.6 \Delta,$$

the linear unit being one mile throughout. The forces of gravity at the two places, are when corrected for their difference of latitude,

$$g \text{ (at Waiau)} = 978.0723 \text{ dynes.}$$
$$g \text{ (at Kawaihae)} = 978.8035 \text{ dynes.}$$

The foregoing values lead to the equation

$$-0.0007470 = -\frac{20.8}{16587.6} + \frac{15.54}{16587.6} \frac{\delta}{\Delta},$$

from which $\frac{\delta}{\Delta} = 0.541.$

If we consider the mountain as a cone with an altitude of 2.473 miles and radius of base (a) of 30 miles, the attraction becomes

$$2 \pi \delta h \left[1 - \frac{h}{\sqrt{h^2 + a^2}}\right] = 14.26 \delta$$

and the resulting value of $\frac{\delta}{\Delta}$ is 0.589.

We may therefore assume that the density of the earth is 1.77 times the density of the mountain, or that the

$$\text{Mean Density of the Earth} = 1.77 \times 2.90 = 5.13.$$

The determination of the force of gravity at a point half way up the mountain gave a means of comparing the relative densities of the upper and lower sections. The data for the three stations are:

	g.	Lat.	h.
	Dynes.	° ′	*Feet.*
Kawaihae	978.8035	20 02 N.	8
Kalaieha	978.4905	19 42	6,660
Waiau	978.0599	19 49	13,060

A small correction for the wear of the knives is here omitted, being the same for all three stations.

These values being corrected for their difference of latitude, and assuming the same value for Δ previously used, gives for the lower section a density nearly twice that of the upper.

Conclusion.

The work on Haleakala is entitled to more confidence than that on Mauna Kea, on account of the method employed, but as the same densities of rock have been used in both cases we adopt the mean. The result of the work is then:

Mean Density of Earth from Haleakala	= 5.57
" " " " Mauna Kea	= 5.13
Adopted mean	5.35

The agreement of the two values is satisfactory when we consider that the methods were entirely different, the former depending on triangulation and astronomical latitudes and the latter on the diminution of the force of gravity from the sea level to the summit as revealed by the pendulum.

List of Determinations, in Chronological Order, of Mean Density of Earth by Methods Depending on Pendulum or Astronomical Observations.

(The mines are indicated by negative depths.)

Mountain or mine.	Country.	Elevation or depth of station.	Observer.	Date.	Mean Density of Earth.
		Meters.			
Schehallien...	Scotland......	1,085	Maskelyne and Hutton.	1775	4.71
Mont Cenis....	Italy..........	1,943	Carlini........	1821	4.67
Harton........	England	— 381	Airy..........	1854	6.56
Arthurs Seat..	Scotland......	251	James and Clarke.	1855	5.32
Fujinoyama...	Japan........	3,792	Mendenhall...	1880	5.77
Pibram........	Bohemia.....	— 1,000	Von Sterneck..	1883	5.77
Freiberg......	Saxony	— 534	Von Sterneck..	1885	6.77
Haleakala	Hawaiian Islands.	3,058	Preston.......	1887	5.57
Mauna Kea...	Hawaiian Islands.	3,981	Preston......	1892	5.13
			Mean......	....	5.59

THE EARLIEST ISOCLINICS AND OBSERVATIONS OF MAGNETIC FORCE.

BY

LOUIS AGRICOLA BAUER.

[Read before the Society November 10,* 1894.]

It may interest the Philosophical Society to know that I have temporarily removed from Washington in order to pursue the study of terrestrial magnetism in European libraries. One of the interesting finds growing out of these studies is that of a rare book by William Whiston,† in the Royal Library at Berlin, containing important matter hitherto wholly overlooked.‡

* This paper was completed August 4, 1894, in Germany, and sent to Professor Cleveland Abbe, in Washington, D. C., who received it October 24, and read it before the Philosophical Society November 10, 1894.

† The book bears the following title: "The Longitude and Latitude found by the Inclinatory or Dipping needle; wherein the Laws of Magnetism are also discover'd. To which is prefixed An Historical Preface; and to which is subjoin'd Mr. Robert Norman's New Attractive, or Account of the first Invention of the Dipping needle. By Will. Whiston, M. A., some time Professor of the Mathematicks in the University of Cambridge. London, 1721. 8vo, xxviii, 115; iv, 43 pp.; 2 maps and 3 cuts."

‡ This paper has been in preparation for some time. Whiston's work first became known to me in the winter of 1892–1893. Early in 1894 I called the attention of Professor Hellmann and Dr. Eschenhagen to it. Since then Dr. W. Felgentraeger, assistant in the Magnetic Observatory at Göttingen, has come across the same work and embodied the results of his careful researches in an article entitled "Die Isoklinen Karte von Whiston," published in Nachrichten Kgl. Gesell. d. Wiss. zu Göttingen, 1894, No. 2, pp. 129–140. As Dr. Felgentraeger's article does not exhaust the contents of the work, I have thought it well to complete this paper and at the same time so modify it as to include a discussion of his interesting contribution.

As appears from the title of Whiston's book, his chief object was to set forth a method of determining the longitude and latitude by means of the magnetic dip needle. The problem of determining geographical positions (chiefly longitudes) by means of the magnetic *declination* is an old one and began with Columbus.* Like the problem of perpetual motion, it was revived and studied from time to time, even down to the beginning of the nineteenth century. But Whiston (1719–'20) proposed to use the magnetic dip for this purpose. He dedicates his attempt to the commissioners appointed by act of Parliament for the discovery of the longitude, and among these commissioners are found the names of Sir Isaac Newton, then president of the Royal Society, and Dr. Edmund Halley, the Astronomer Royal.

Whiston was led to his method by examining Halley's isogonic chart of 1700.† As is well known, this is generally conceded to be the earliest published chart of the lines of equal magnetic declination—*i. e.*, those lines connecting all the places on the earth's surface at which the magnetic needle makes the same angle with the true north-and-south line. By reason of this chart, Halley is usually credited with originating the general method of representing the distribution of any phenomenon on the earth's surface by lines drawn through all those places where the phenomenon in question has the same numerical value. It appears that others before Halley had conceived this idea, but, though they may have carried it out privately, thus far no earlier

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* Columbus has not been fully credited in America with the discovery of the magnetic declination. In Europe, through the researches of Bertelli, Peschel, and others, he long ago received this distinction. For several years the writer has been searching for everything pertaining to early magnetic observations. Although he has had access to many rare old works, he has thus far found nothing indicating a knowledge of or a recognition of the magnetic declination on charts before Columbus' time.

† Published separately in London, 1701, and reproduced photo-lithographically in Greenwich Observations, 1869; now about to be republished in "Neudrucke von Schriften und Karten uber Meteorologie und Erdmagnetismus." Herausgegeben von Prof. Dr. G. Hellmann. Berlin, A. Ascher & Co.

publication has come to light. Lines of equal magnetic declination are hence frequently called Halleyan lines. Halley's method, though in a certain sense artificial, is yet applicable for many purposes, and has been frequently applied, not only to magnetic, but to many other phenomena.

The representation of the distribution of the magnetic dip or inclination by the Halleyan method—*i. e.*, by isoclinics—has hitherto been credited to Wilcke, who issued his isoclinic chart in 1768.*

Isoclinics have, therefore, also received the name of Wilckean lines.† In the following it will be shown that Wilcke cannot properly be given priority in originating isoclinics; hence the term "Wilckean" applied by Hansteen is not justifiable and this honor must be given to Whiston.

By studying the Halleyan lines Whiston found that from their irregularity and "the Quickness of the Mutations of those Lines and their different Position in the rest of the World" they could not be satisfactorily used for determining longitude; hence he was led to consider "Lines of equal Dip," thinking these would serve his purpose better (see p. xxvii). To this end he collected all observations of dip made up to his time, and by means of them drew upon a Molyneux terrestrial globe the lines of equal dip as far as was then possible. He mentions this on page 53, but unfortunately neither gives *the collected observations* (which were those made by Hudson, Noel, Pound, Cunningham, Feuillee, and Windham), nor does he reproduce these isoclinics in his book. Further proof of his right to be regarded as the

* Försök tel en magnetisk inclinations-charta. Vetensk. Akad. Handl., Stockholm, 1768, or German translation in K. Schwed. Akad. Abth. xxx, p. 209, Leipzig, 1768. Wilcke's article makes such mention of some of the contents of Whiston's book that it seems to me probable that he was familiar with Whiston's work and got his idea of isoclinics from it. While he nowhere mentions Whiston's isoclinics, he also nowhere lays claim to originating lines of equal dip. Still to Wilcke belongs the credit of having first *published* a general isoclinic chart. Hansteen's isoclinic chart for 1700 is chiefly made up from Wilcke's.

† Called so by Hansteen in his "Magnetismus der Erde," Christiania, 1819. Hansteen evidently had not seen Whiston's book.

originator of these lines, which he continually refers to as "Lines of equal Dip," and even terms "Magnetick Parallels,"* consists in the fact that his book contains two small maps facing page xxviii, showing the lines of equal dip for every quarter of a degree for southern England and northwestern France.

These lines were based upon observations chiefly made by Whiston himself for the express purpose of drawing the isoclinics to be used in his method of determining geographical positions. The first map shows the isoclinics resulting from dip observations made in 1719, with a needle 12 inches long, and embraces a district lying between latitude 48½° north and 55¼° north and longitude 2°.5 east and 2°.5 west of London. The second,† covering the region lying between latitude 49° north and 54° north and longitude 2° east and 3° west, shows isoclinics based upon more numerous observations made with a 47½-inch needle.‡

The values at the various places are also tabulated on this second map. The results from the two needles differ, on an average, by about 1°.5, the longer needle giving the larger value. To counteract the error due to flexure of the long needle, Whiston placed a small "Poise of brass circular Wire" on the north end of the needle, which required shifting to and fro according to dip. He also says (p. 49) that the results with the 12-inch needle were too small, and that those with the long, 4-foot needle, agreed with those of the small needles (4½ and 6 inches), but he nowhere gives a comparison.

Whiston believed that the larger the needle, the better the result, if the needle be "poised" as stated; but experience has not supported this belief, which appears to rest upon a false assumption. Dr. Felgentraeger in his article adopts

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* See pp. 53, 55, and 58. On page 53 he defines the magnetic meridians as the "Perpendiculars to the Magnetick Parallels of equal Dip." On page 54 he speaks of "Magnetick Equator."

† Dr. Felgentraeger has given, in his article, a fac-simile reproduction of this second map.

‡ See Whiston, pp. 91 and 92.

the results obtained with the $47\frac{1}{2}$-inch needle. It appears to me, however, that the results with the latter needle were 0°.5 or more too high, while those with the 12-inch needle are about the same amount too low, and consequently the mean result from the two needles lies nearer the truth. This may be seen from the following considerations: On page 28 Whiston gives the time of one small oscillation of a 4-foot dipping needle in the plane of the magnetic meridian at London as 22 seconds. On his second isoclinic map the time of one horizontal vibration at London and at about 120° * from the magnetic meridian is given as $60\frac{1}{4}$ seconds.†

Now, the squares of the times of vibration of a horizontal needle in different planes are to each other inversely as the cosines of the magnetic azimuths of the planes. Hence, to reduce the second observation above so as to apply to the magnetic meridian, we have the following proportion:

$$(60\tfrac{1}{4})^2 : x^2 :: \cos 0° : \cos 60°,$$

whence

$$x = 60\tfrac{1}{4} \sqrt{\cos 60°} = 42.6 \text{ seconds.}$$

With the aid of the two vibration times, 22 seconds and 42.6 seconds, we can now compute the dip at London at that

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* As the magnetic declination at London was at that time about 13° west of north, this angle probably was counted from north towards west.

† Whiston appears to have had two needles about 4 feet long. One was $47\frac{1}{2}$ inches long, and weighed 4,015 grains before magnetizing and $4,012\frac{5}{8}$ grains after magnetizing. The other was 4 feet long, and weighed $4,584\frac{1}{8}$ grains before magnetizing and $4,012\frac{5}{8}$ (sic) grains after magnetizing. The needles were of a "prismatick or cyclindrick figure." The "poise" to counteract flexure of the long needle was, in the case of the shorter one, 1.77 grains, and of the other 2.44 grains. This poise had to be shifted along the end of the needle dipping below the horizon to a position depending upon the angle of inclination, and as this could be done only roughly, since the angle of inclination had first to be determined, it is apparent that no such reliance is to be put upon the results with the 4-foot needles as Dr. Felgentraeger appears to attach to them. I could not ascertain from Whiston's book with which one of the two long needles he observed the vibration times given on his second isoclinic map. On page 38 he says, however, that the "time in both my Needles of 4 Foot long is about 22 seconds;" so that we may assume without great error that the vibration times all refer to the same needle.

time according to the well-known method for determining dip by means of the vibration time of a dipping needle in the plane of the magnetic meridian and the vibration time of the same needle swung horizontally. This method appears to have been first invented by Whiston, and is given by him as an approximate control upon the usual method. Hence, if F be the total magnetic force, we have the following relation:

$$F : F \cos dip :: 42.6^2 : 22^2,$$

or

$$\cos dip = 22^2 \div 42.6^2 = 0.2667;$$

whence

$$dip = 74^\circ\ 32'.$$

Now, the mean of the results of the one-foot needle, $73\frac{3}{4}^\circ$, and of the four-foot needles, $75\frac{1}{6}^\circ$, is $74^\circ\ 27'.5$.

A preliminary interpolation formula (Science, vol. xx, No. 506) established by myself in 1892 on the basis of the inclinations observed at London from 1576 to 1888, viz:

$$I = 70^\circ.40 - 3^\circ.98 \sin \left[0^\circ.7\,(t - 1850) + 23^\circ.02\right],$$

where I is the inclination at the time t, gives for 1720.5 $I = 74^\circ.1$. Graham obtained $74^\circ\ 42'$ as the result of observations March 29–May 2, 1723.

It seems to me better, therefore, to adopt the mean results from the two needles, rather than those from the four-foot needle alone. This course gives for London in 1720.5

$$dip = 74^\circ.46 \pm 0^\circ.5.$$

From the above we might conclude that the *absolute* value of either of the Whistonian isoclinic maps is impaired to the extent of about 1°. The *relative* value, however, remains and is affected either not at all or but slightly by the constant instrumental error—*i. e.*, the *direction* of the isoclinics over England in about 1720 may be accepted as given by Whiston. This direction is approximately WNW. to ESE.; the present direction, roughly, WSW. to ENE. If

Whiston's directions are fairly trustworthy, they will furnish a valuable contribution to our knowledge of the secular variation of the dip over England. In the accompanying sketch the mean isoclinics are shown by full lines for the epochs 1720, 1837, 1860, and 1886, as drawn by Felgentraeger. It will be seen that between 1720 and 1837 the isoclinics moved anti-clockwise and are now moving in the contrary direction. It would be interesting to determine when this motion was reversed. The first one to call attention to this retrograde

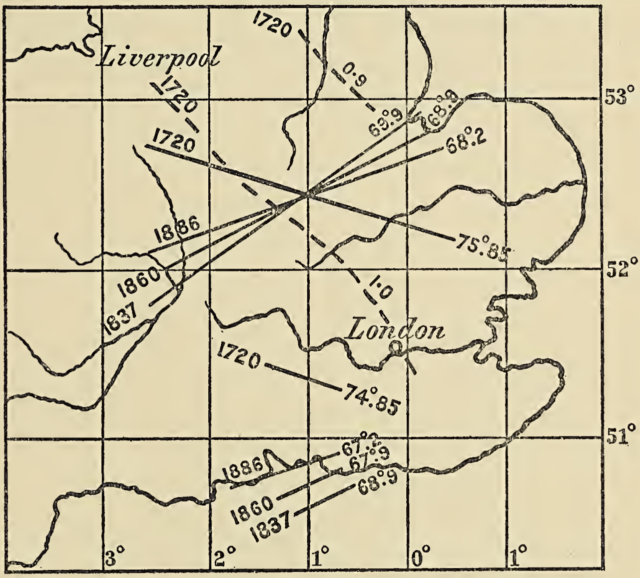

Fig. 18.

motion was, I believe, Sabine,* who says: "The angle of intersection of the meridian and isoclinics has been diminishing up to about 1840, when a reversal took place, and the angle is now increasing." So the position for 1837 is probably not far from the lower western extreme position. The angle of shift between 1720 and 1837 appears large. Doubtless enough observations made between 1720 and 1837 may be found to enable us to draw intermediate positions of the

* Sabine (Sir Edward). Proc. Royal Soc., 8°, London, 1862, vol. xi, p. 144.

isoclinics. As the observations so found will rarely apply in sufficient number to the same epoch, use may be made of the London secular variation formula, as above given, for reduction to a common epoch. I hope this matter will incite British terrestrial magnetists to pursue the subject further. It would be a valuable addition to our knowledge of the secular variation if all the observations ever made in England were carefully collected and discussed.

Whiston comes in, however, for a still greater share in the early terrestrial magnetic discoveries, and this has likewise been overlooked. It is usually asserted and believed that the earliest observations of relative intensity of terrestrial magnetic force are those of Mallet (1769), or the more successful ones of Lamanon (1785–'87). Mallet's observations were made at two stations only, St. Petersburg and Ponoi. Little importance appears to have been attached to them even by Mallet himself. As he found the times of vibration of his needle the same at both places, it was inferred that the magnetic force was the same at all points on the earth's surface. Lamanon's observations, the record of which appears to have been lost in shipwreck, were more successful and yielded the important discovery that the attractive force (horizontal) of a magnet is less in the tropics than towards the poles, and that the total intensity increases with the latitude. In consequence Humboldt has credited Lamanon with the discovery of this law, which was further confirmed by Humboldt's own observations, 1798–1803. Of observations fairly trustworthy, these by Humboldt have been hitherto regarded as the earliest; but this distinction should be awarded, as I think, to Whiston, as appears from the following: *

* It may be remarked here that the earliest observations with respect to the *annual* variation of the intensity appear to be by Musschenbroek (Phil. Trans. Roy. Soc., No. 390, p. 370, 1725). "He wished to observe whether the force of the magnet was the same every day or greater or less in summer than in winter, but he found by several experiments that the force is less in summer than in winter, at least in the summer of 1725."

We have already referred to the "vibration time" of a horizontal and of a vertical magnet at London. When Whiston observed the dip with his great four-foot magnets at various places in England in 1720 he also observed the time of one horizontal vibration of these magnets at those places for the express purpose of determining the difference of strength of the magnetic force in different parts of England. Apparently the results were not as accordant as he expected, for he says almost nothing about his method. On his second isoclinic map he records the observation which are reproduced in the table below. The only explanation of them is the following passage on page 112: "The Difference of this "*Strength* of the Magnetick Power, from its *Direction*, is most "visible in my Second Map, hereto prefix'd: Where I have "all along set down the Seconds wherein my Needle per- "form'd a single horizontal Vibration at about 120 Degrees "from the Magnetick Meridian, in most Places: Whose "Squares, when Allowance has been made fer the different "Obliquity of the several Directions as to our Horizon, will "give us the different *Strength* of that Magnetic Power at "those several Places: As does the Angle of Dip give us the "different *Direction* of the same Power there. Now at the "first Sight, the former there appears to be irregular, and the "latter regular; as is the Case also of our *Terrella*."

Now, our problem is: How did Whiston observe the time of one horizontal vibration 120° away from the magnetic meridian and yet have acting on the magnet no other force than the terrestrial magnetic component? Why did he observe in a plane the magnetic azimuth of which was 120°? Why did he not simply observe the time of vibration of the dipping needle in the magnetic meridian in order to compare the force at various stations, just as he had done at London? These questions are hard to answer, and are submitted for the consideration of physicists.

Before the Whistonian vibrations can be intelligently discussed it must be settled whether Whiston really did what I have indicated. Are the vibration times given such as

would be obtained in the horizontal plane 120° (probably counted from north through west) from the magnetic meridian? We can answer this in the following manner: The time at London of a single horizontal vibration of one of the four-foot magnets, at 120° from the magnetic meridian, is given by Whiston as $60\frac{1}{4}$ seconds. On page 401 of this present article we found that this corresponds to 42.6 seconds for the freely suspended magnet. On the other hand, one vertical vibration of the same or a similar magnet in the plane of the magnetic meridian is given as 22 seconds, while the corresponding dip was observed to be $75\frac{1}{6}$°. Hence, from the following relation

$$F : F \cos 75° \, 10' :: x^2 : 22^2,$$

we get x = time of one horizontal vibration as deduced from dip observation and time of oscillation of dipping needle = 43.4 seconds, whereas the reduced horizontal vibration yielded 42.6 seconds. Therefore we may conclude that the vibration times given by Whiston on his map really apply to a plane 120° from the magnetic meridian, and that they represent the strength of no other force than the terrestrial magnetic component.

The next question is: What reliance can be put upon these observations for determining the distribution of terrestrial magnetic force over England in 1720? Their absolute value we, of course, cannot determine.*

Each observation is doubtless affected by a large probable error, owing to the omission, by Whiston, of numerous precautions needful in such observations. Nevertheless it can be shown that these earliest relative intensity observations possess a value that deserves recognition. In the first place, we have already (on page 402) applied a rough control in that we, with the aid of the observed vibrations, obtained a value

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* On page 22, in connection with observations for determining the law of attraction between magnets, Whiston speaks of having made deflection observations with needles. He does not, however, give the obeservations; had he done so, perhaps a rough value of absolute force at London in 1720 might have been obtained.

of the dip agreeing well with the observed values. Secondly, if we draw the isodynamic lines with the aid of the observed vibrations, we find that they run approximately parallel to the isoclinics, just as they should, and thus mutually confirm each other.* Thirdly, the vibration time for London (latitude 51°.5 N., longitude 0°.12 W.) is 60¼ seconds, while that for the most northern station, Saltfleet (latitude 53°.4 N., longitude 0°.19 E.), is 66 seconds. Hence for two stations 2° apart in latitude and nearly in the same meridian, and through which consequently the same isodynamic line would not be likely to pass, the difference in the vibration times is 5¾ seconds. Now, as the force varies inversely as the squares of the vibration times, the London and Saltfleet observations show that the horizontal force is less at Saltfleet than at London, as is the fact. Thus it appears that Whiston's observations for relative intensity are consistent among themselves and with later and improved results.

Whiston also endeavored to compare the force of gravity with that of terrestrial magnetism, but by mistakes in reasoning and computing obtained erroneous results.

In the following table I have given Whiston's observations of dip and intensity. The geographical positions of the stations are taken from Dr. Felgentraeger. The stations are arranged according to increasing latitude, but I have indicated by numbers the order as given by Whiston, which may possibly be the order in which the observations were made. The resulting dips with both needles are given wherever possible. The seventh column gives the results that I would feel inclined to adopt from the foregoing considerations. These results are the means of the two sets of observations. Where the observation of the long needle only was at hand, a correction that varied from —0°.6 to —0°.9, as obtained from those stations where observations with both needles are given, was applied in the order of time as indicated by

*On the sketch (page 403) I have roughly drawn the lines of equal relative horizontal intensity, taking London as unity. The broken lines represent these relative isodynamics. They were sketched with the aid of the last column of figures in the table on page 408.

the number in the first column. The probable error of these adopted results may be roughly given as ± 0°.5.*

Whiston's number.	Stations.			Inclination.			Horizontal vibration for 1720.		Relative horizontal force for 1720. London = 1.
	Name.	Latitude.	Longitude.	12-inch needle, 1719.	47½-inch needle, 1720.	Adopted for 1720.	T 120°.	T 0°.	
		°	°	°	° ′	°	Sec.	Sec.	
24	Kew	51.47	0.31 W.	....	75 09½	74.6	....	....	....
25	Greenwich	51.48	0.00	....	75 14½	74.5	....	....	....
22	Windsor	51.48	0.59 W.	....	75 05	74 3	....	....	....
23	London	51.52	0.12 W.	73¾	75 12	74 5	60¼	42.6	1.000
26	Wanstead	51.58	0.04 E.	....	75 19	74.7	....	....	....
21	Dunstable	51.88	0.50 W.	....	75 24	74.7	59¾	42.2	1.018
20	Ainhol	52.00	1.27 W.	74	75 25	74.7	59⅓	41.9	1.033
19	Banbury	52.06	1.33 W.	....	75 25	74.7	60	42.4	1.009
18	Maidford	52.20	1.22 W	74⅛	75 37	74.9	59⅔	42.2	1.018
17	Lichborough	52.22	1.19 W.	....	75 37	74.8	60	42.4	1.009
16	Northampton	52.25	0.91 W.	....	75 44	75.0	60	42.4	1.009
15	Harrington	52.40	0.89 W.	....	75 56	75.2	59	41.7	1.044
8	Leicester	52.63	1.15 W.	....	76 06	75.3	62	43.8	0.946
10	Tamworth	52.63	1.64 W.	....	75 49	75.1	61½	43.6	0.954
9	Bosworth	52.64	1.36 W.	....	76 00	75.2	59½	42.1	1.024
7	London *	52.66	0.73 W.	74½	76 14	75.4	63	44.5	0.916
11	Rugely	52.77	1.93 W.	....	75 52	75.1	62	43.8	0.946
5	Barkston	52.93	0.62 W.	74¾	76 32	75.6	64	45.2	0.887
6	Belton	52.94	0.65 W.	....	76 40	75.8	63⅔	45.0	0.896
12	Oure	53 00	2.53 W.	....	76 05	75.3	60	42.4	1.009
13	Chester	53.19	2.89 W.	....	76 22	75.4	59¼	41.9	1.033
4	Lincoln	53.1	0.45 W.	....	....	....	64	45.2	0.887
3	Wragby	53.3	0.15 W.	....	....	....	64¼	45.4	0.880
2	Louth	53.35	0.10 W.	....	....	....	66	46.7	0.831
14	Liverpool, 8 miles west of	53.41	3.12 W.	....	76 34	75.8	....	....	....
1	Saltfleet	53.42	0.19 E.	....	77 15	76.4	66	46.7	0.831

* Whiston gives the magnetic declination at London as 17° 15′ W. for 1720. Whether this is an observed quantity cannot be ascertained.

The eighth column gives the observed horizontal vibrations 120° from the magnetic meridian with a 4-foot needle, as given by Whiston on his second map. The next column contains the horizontal vibrations, as reduced by myself to the magnetic meridian. The last column represents the distribution of the relative horizontal force, taking the value at London as unity. The figures were obtained from the foregoing column with the aid of the rule that the force varies inversely as the squares of the vibration times.

* In case these inclinations are adopted the isoclinics in the sketch on page 403, as drawn by Felgentraeger, would receive a constant correction of about — 0°.8. The relative value of the isoclinics would, of course, not be affected.

To Whiston, then, is due the credit for the following three achievements:

1. He drew the first isoclinics and invented the name "magnetic parallels" for them.

2. He made, in 1720, the first relative intensity observations.

3. He invented the vibration method for determining the dip.

FRIEDENAU, BEI BERLIN, *August* 4, 1894.

NOTE.

The following extracts kindly communicated to me since writing the above paper are deemed of sufficient importance in connection with this paper to warrant their insertion here. They are extracted from a book entitled

"Memoirs of the Life and Writings of Mr. William Whiston, containing memoirs of several of his friends also." Written by himself. London, 1749.

Volume I, page 292: About this Time, 1720, I printed and gave away, to some of my mathematical Friends, a few Copies of a small imperfect *Essay on a Discovery of the Longitude by the Dipping Needle.* But because I afterward made many and great Improvements in that Matter, and published the whole in a much larger Treatise, a Year or two afterward, upon that Subject; of which presently; I drop this first Essay intirely.

Volume I, pages 296–'7: In the Year 1721, I published *The Longitude and Latitude found by the Inclinatory or Dipping Needle;* Wherein the laws of Magnetism are also discovered. To which is prefixed an *Historical Preface;* and to which is subjoined Mr. Robert *Norman's New Attractive,* or Account of the first Invention of the Dipping Needle, price 2*s.* 6*d.*

N. B.—After the Publication of this Treatise, I found so much Encouragement from many Benefactors, that I was enabled to procure some New Observations of the Angle of Dip in several Parts of the World, in order to perfect this Discovery; the Substance of which is printed at the End of my *Calculation of Eclipses, without Parallaxes;* of which presently. Which upon the whole cost me a very great deal of Pains, to contrive the Instruments and hang them in Ships so as to take the Dip, with an Exactness sufficient for my Purpose; but found the Power of Magnetism so very weak, and the Concussion of a Ship so very troublesome, that I had little Hopes of succeeding. And when I knew of Mr. *George Graham's* new Discovery of an *Horary* uncertain *Inequality*, as I may call it,

both in the Variation and Dip of Magnetick Needles, in No. 383 of the Philosophical Transactions, and this as far as half or two-thirds of a Degree, if not sometimes of a whole Degree (which last Quantity I once observ'd myself in a Dipping Needle of my own, of almost four Feet long, in the Space of Eight Hours) I perceived that all my Labour was in vain, and I was obliged to drop that Design intirely.

Vol. I, pages 315-16: In the same Year 1724, I published the Calculation of Solar Eclipses without Parallaxes; with the Discovery of the Geographical Longitude of Places by such Eclipses. And an Account of some Observations made with Dipping Needles, 8vo. Price 1*s*. 6*d*.

N. B.—This Book has so many Mistakes, that 'till they are corrected I do not desire to have it spread abroad any longer. I am myself now too old to take Pains in the Review; And as I have heard Sir *Isaac Newton* say, that no old Men (excepting Dr. *Wallis*) love Mathematicks; I so may well be excused here, especially when I have been long so busy about Things of much greater Consequence.

On examining Whiston's work of 1724, I find some information as to the dip at Boston, U. S. A., in 1722, and with this I can extend the secular curve for Boston back to that date.

L. A. B.

FRIEDENAU, BEI BERLIN, *November* 13, 1894.

ANATOMY AND ART.

BY

ROBERT FLETCHER.

THE ANNUAL PRESIDENTIAL ADDRESS,

DELIVERED

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The question whether art in its early development was at all aided by anatomy, and the further inquiry whether the aid of anatomy is of any real benefit to art, are not new questions. Critics and artists have expressed their opinions on one or the other side. Mr. Ruskin, with characteristic vehemence, has returned a decided negative to both questions. Writers on art, being human, are prone to repeat dogmatic rules, and so the assertion is very commonly made that the artist who proposes to represent the human form, whether in sculpture or in painting, must in part fit himself for his work at the dissecting table. It is the purpose of this address to endeavor to examine the matter from a historical point of view chiefly. The rise and progress of the study of anatomy, and its rapid advance by means of artistic illustrations after the invention of printing, considered in relation to art in its earlier age and its later development, will, perhaps, assist in an understanding of the subject.

While anatomy may be defined generally to be the study of an organized body, there are many recognized divisions of the subject, one only of which concerns the present inquiry. Artistic anatomy comprehends the whole external

form of the body with the superficial veins of the skin and the muscles immediately under it. It must also be understood to include certain external pathological conditions, for the leper and the demoniac, the lame and the blind, are frequently introduced in early Christian art and in the paintings of subsequent times. A better term than artistic anatomy, more comprehensive as implying form as well as structure, would be *artistic morphology.*

It must not be supposed that artistic anatomy, or morphology, implies only the study of the dead body. The flaccid inexpressive muscles can teach little except their mechanical attachments and relations. It is function as well as structure—physiology as well as anatomy—which is of as much, or of more, importance to the artist. Some of the differences of opinion on the question at issue are doubtless due to the neglect of this distinction. Another vital point of artistic morphology is the study of the modifications which are seen in external form from the influence of sex, age, and race. Holman Hunt made a long sojourn in Judea before painting his famous picture of Christ among the Doctors. Nevertheless, in spite of his close observance, a young lady made this comment: "It is very fine, but one can see that the painter was not acquainted with the distinctive character of the tribe of Judah; he has given to his doctors the flat feet of the tribe of Reuben, whereas the men of Judah had highly arched insteps."

Occupation, too, puts its distinctive mark upon the human figure, and certain joints or muscles become permanently enlarged or disfigured.

It has been already said that the artist should know something of pathology before attempting to paint the effect of disease. In like manner, he must learn how the muscles can assume a most pathetic expressiveness when death is imminent from a mortal injury. It has been objected to that exquisite statue, The Dying Gladiator, that the pose is somewhat theatrical—that the dying man should be prone or supine on the ground. Not at all—the two-edged Roman

sword has pierced his chest, the lungs are filling with his life's blood, and with shoulders drawn forward he leans upon his arm, gasping for breath. He may be thinking of "his young barbarians," and of "their Dacian mother," far away, but his position in the death agony is strictly natural.

There are those who regard with some degree of timid dread the introduction of science into art. Art, they affirm, is conventional and full of exaggerations, and has nothing in common with the precision of science. A great master who was not only painter, but sculptor, architect, engineer, Leonardo da Vinci, has clearly defined this matter. "In a general way," he says, "it is the office of science to distinguish what is impossible from what is possible. Imagination left to herself would revel in unrealizable dreams. Science controls her by showing what cannot be. It does not follow that science includes the principle of art, but that we must study science either before or at the same time with art to learn in what limits the latter is to be confined."

No doubt a knowledge of anatomy cannot make the artist, any more than study and training can make the poet—the divine vocation must be there—but spontaneity in either is a mere euphemism for indolence, and the world is very likely to pass by such work. Art is serious, and is not to be regarded as a pastime merely.

The critic in art would, in like manner, be somewhat assisted by a knowledge of anatomy, for patient study must be his dependence, and not a mere reliance on taste. A susceptibility of pleasure at sight of a work of art, constituting *taste*, is popularly supposed to be a sixth sense, and it has been satirically said that every man believes himself to be possessed of it. This, after all, is not surprising. A man will readily acknowledge ignorance of what it obviously requires time and labor to acquire, but not to possess an inborn sense of taste would be to renounce a birthright and to take rank with the outcasts of Nature! Vanity is perhaps at the bottom of it all:

"Some demon whispers 'Strephon, have a taste.'"

Whether in art or literature, cultivation of the critical faculty demands long study, close observation, and familiarity with the best masters. It is not to the point to say that the critic cannot paint the picture, carve the statue, build the temple, or write the poem. If he could, there would be an end of criticism, and the world would be more full than now of crudities in art.

It may be boldly said that a close study of the anatomy in a great work of art is a part of a fundamental error in art criticism, namely, the too minute consideration of the process of production rather than the appreciation of the life and harmony of the whole. The *technique* must not deprive us of the artistic illusion. It is in archaic art that we are impressed with the building up of the body. Flexibility is wanting, and it is in high art only that we find dynamics in place of statics.

The influence of anatomy is of more importance to sculpture than to painting. In the latter a surface only is presented, while in a statue the whole figure must be displayed. Clothing, which exhibits the most exquisite effects of the painter's art, is stiff and ungainly in the dull marble, and has always been discarded as much as possible by the sculptor. Herr Teufelsdrockh could never have thought out his philosophy of clothes in a gallery of sculpture.

Artistic anatomy in a master hand greatly assists the understanding of the story intended to be portrayed. It was said of a very famous picture that there was a story in every joint of the fingers of the principal figure. If a story is to be told, however, it should be recounted in poetry or prose. It has been shrewdly observed that if the famous Angelus of Millet were to be looked at hundreds of years hence, when church bells had long since been disused and were forgotten—if that blessed time should ever arrive—no comprehension of the meaning of the painting would be possible.

It is a noteworthy fact that the most persistent advocates of the essentialness of anatomy to art speak only, as a rule

of the human figure, and yet the argument must apply equally to animals. Can we suppose that either of the three Rhodian sculptors ever dissected a python to enable him to carve the graceful folds of the two mighty serpents which are crushing the agonized Laocoon and his two hapless sons? Did Barye obtain his knowledge of the musculature of the tigers and elephants of his wonderful groups from the dissecting table? Look at Meissonier's famous painting of the Portrait of the Sergeant. The dog who is looking up at the martial pose of the soldier with such a diverting expression of wonder is a mere mass of wiry hair, with hardly a suggestion of bone or muscle. External form only is visible. A study of the works of the great animal painters and sculptors will show the impossibility of dissection having been a prerequisite to their art.

A famous statuary, whose horses are especially admirable, tells me that he never knew an artist much given to the study of anatomy who did not spoil his work by over-minute anatomical detail. On the other hand, he observed, a certain knowledge of the anatomy of the horse from drawings or models is desirable, or the artist may copy a wind-gall for a muscle!

Ruskin, in his lecture on the relation of art to organized form, with some exaggeration pronounces anatomy to be not only useless to the artist, but to be a positive injury. He instances the works of Albert Dürer, whose knowledge of anatomy was so minute and extensive that he was never able to produce a beautiful human face; the bones, of which he knew too much, were in his way.

There is nothing to indicate that oriental nations, or the ancient Greeks, knew anything of muscular anatomy, except from external form. Their warlike pursuits made them well acquainted with the viscera through the effect of wounds. The names of organs and regions of the body mentioned by Homer, and which have mostly come down to us through Hippocratic writers, number over one hundred and forty according to Daremberg, and many of them

are the accepted technical terms in present use. In the Vegas such allusions are common, but generally absurd.

In ancient Egyptian art the human figure is very defective. Quite lately, however (October, 1894), a statuette of very dark hard wood, which was discovered in the Therapeum at Karnak, has been deposited in the Louvre. It has been ascertained that it represents a priestess of Ammon, named Touïe, who lived some 3,500 years ago. The figure is that of a Nubian girl, and is exquisite in its proportions. In anatomical correctness it is equal to the famous "Crouching Scribe" in the same collection, which belonged to the fifth dynasty and is, therefore, between five and six thousand years old. It would be interesting to know the history of these rare exceptions. There was a law in ancient Thebes, it is said, by which the artist who produced a sculpture-portrait worse looking than the original was heavily fined. The temptation to beautify the latter must have been great, but the marble does not admit of as much flattery as the canvas.

In early Assyrian art the anatomy of the human figures portrayed is invariably bad, but the curious anomaly exists that sculptures of animals are frequently spirited and correct. Why the power of imitating, so excellent in the one case should so fail in the other, does not seem to admit of explanation.

It is nearly certain that Hippocrates never dissected a human body, and the extensive investigations of Aristotle were confined to animals, birds, and fishes. The practical study of human anatomy may be said to have arisen in Egypt under Ptolemy Sotor, and Herophilus and Erasistratus were permitted to dissect bodies. Their writings are lost, and it is through Galen and Celsus that their names and some notices of their work have been preserved.

To Galen must be given the great praise of having brought together all that was known in his day on anatomy, and of having added an immense number of observations of his own. Nevertheless, it is admitted by the best critics

that Galen probably never dissected a human body. His descriptions have been followed with painful minuteness by Daremberg, and the conclusion is irresistible that most, if not all, of his dissections were made upon monkeys. Galen's name was most authoritative in all matters of anatomy and physiology for many centuries, and the blind reverence of his disciples was exemplified in the reply of Silvius when being forced to admit the inaccuracy of one of Galen's descriptions, he said that "Man had changed and not for the better."

Art was far in advance of medicine. The noble works of Pheidias and his contemporaries or successors were in existence long before the time when Hippocrates began the work of rescuing medicine from the priests and made a first imperfect sketch of anatomy. For many ages after him anatomy was, in the words of a clever writer, "traditional and, in great part, fictitious; physiology little more than a farrago of crude conceits and baseless dogma; medicine a dismal combination of empiricism, superstition, and filthy charlatanism, tinged with a slight infusion of the black art." *

It is probable that the oldest anatomical drawings known to exist are to be found in a Persian manuscript. It is a treatise on anatomy by Mansour-ben-Ahmed; it is dedicated to Mirza pir Mohammed, a grandson of the famous Tamerlane, and who died in the year 800 of the Hegira, or 1406 of our era. There are six figures, of which one is a skeleton, having five false ribs on the right side and only four on the left—in verification, no doubt, of the story of the birth of Eve, for early Persian literature has frequent allusions to Moses and the writings ascribed to him. Another figure has the chest well opened, displaying the heart and lungs. This was a favorite subject for drawing with the early anatomists. The compactness, contrast of color, and importance of these organs made them especially fit to copy.

*Art in its relation to medical science. William Anderson. St. Thomas Hosp. Rep., n. s., XV, 1886, 151-181.

There is a relation of anatomy to art which is seldom referred to in didactic treatises, and that is the true delineation of deformity. Nature is never inconsistent, but is harmonious even in deformity. "For example," says Diderot, "a wry nose is natural, if it does not offend us; we are led up to the deformity by little adjacent alterations, which tone it down and belong to it. But twist the nose of the Antinous, leaving the rest of the face unchanged, and you have an unnatural effect. The Antinous will not have a wry nose, but a broken nose." An example of another kind of incongruity is to be seen in a picture by Burne Jones, where solemn faces, which remind one of Fra Angelico's "embodied ecstacies," are joined to the bodies of joyous young bathers.

The anatomical modifications produced by deformity or disease, and which are so admirably displayed in many of the paintings of the old masters, have been the subject of an able essay by the late Professor Charcot. He shows that photographs of convulsionists and hysterical epileptics strikingly recall the representations of those "possessed of devils." The earliest known drawings of an exclusively pathological character are found, strange to say, in a Japanese manuscript of the twelfth century, a copy of which is in the British Museum.* Various forms of disease of the skin are very well represented, and would seem to have been drawn from life. The most noteworthy figure is that of a man whose mouth appears to be obliterated, and who is introducing food through an aperture in the region of the stomach. A gastric fistula, the result of accident, as in the well-known case of Alexis St. Martin, or produced artificially in cases of stricture of the upper part of the alimentary tube, or for the sake of observing the process of digestion in the laboratory, is supposed to belong to the present century. So here is again an opportunity for moralizing!

Very few remains relating to anatomical art have been discovered of Roman origin. A marble figure of great in-

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* Anderson.

terest was dug up in Rome in the grounds of the Villa of Musa, who was physician to the Emperor Augustus. It is a human torso, but the front of the chest and abdomen has been removed so as to expose the viscera. The heart is situated in the very center of the thorax, and is vertical in position. It exactly corresponds to the heart described by Galen, but which we know was studied on the ape. The lungs have three lobes on the left and two on the right. The chest is human, but the contents simian. It is probable that this curious figure was constructed for teaching purposes, and the crudity of the carving would correspond to the time assigned to it, long before the appearance of Galen's work.

A huge gap has to be surmounted at a bound from the time of these relics of classic art to the period when the invention of printing gave a marvelous impetus to the study of anatomy. Some of the earliest sources of illustration for anatomical teaching were *nielli*. The beautiful work known as *niello*—an Italian word equivalent to the Latin *niger*, or black—was produced by melting a black amalgam into the etchings on a metal plate, generally silver, which was afterward highly polished. The most delicate tracing that the instrument of the engraver could produce became permanently black and formed an exquisite contrast to the lustrous silver which it adorned. "Rubbings," as we should term them, were obtained from *nielli* exhibiting nude figures which were made use of by the early teachers to illustrate their lessons on the form of the human body.

With the invention of carving on wooden blocks and engraving upon copper plates anatomical drawings began to assume book form. Among the earliest of which we have any knowledge are two quaint woodcuts in the *Fasciculus Medicinæ* of John Ketam, a German physician, whose work was published in 1491.

In the 14th and 15th centuries a lesson of anatomy on the cadaver was an event of such great rarity and importance that it was announced throughout the university

where it was to take place, and lords and nobles, even, were invited to be present. A curious manuscript of the 14th century, which belonged to Guy de Chauliac, a famous French surgeon, and which is preserved in the Library of the Academy of Medicine of Montpellier, gives a striking representation of such a lecture. Women, too, were present, a fact which may be comforting to some of my hearers.

The limits of this address will not permit of a detailed description of the early crude production and the gradual improvement of anatomical woodcuts and engravings. I shall come at once to the great man who first in all the ages, discarding animals, dissected the whole human body itself, and made known his discoveries by masterly plates which excite admiration to this day.

Andreas Vesal, or Vesalius, was born in 1513 or 1514, in Brussels. In his student days anatomy was taught in this wise: The professor sat in his rostrum with much dignity, reading from Galen, while his assistant, sometimes a barber surgeon, sometimes only a servant, armed with a razor, made incisions under the direction of his master. Dogs and pigs were the ordinary subjects for dissection. Human bodies were rarely obtained. Vesalius relates that he only saw three during the three years of his studies in Paris. Such an examination was almost limited to an inspection of the viscera, and there seems to have been no demonstration of the muscular system.

When Vesalius became professor of anatomy and surgery at Padua his fame as a master of human anatomy attracted students from all parts of Europe. He had early discovered that Galen's knowledge of anatomy was the result of the dissection of monkeys, which animals that writer had supposed to exactly resemble man. His bold announcement of the errors of the teacher so long held to be an infallible authority brought upon him severe animadversions from those who worshipped Galen and were, beside, jealous of the daring young innovator.

His great work was published at Basle, in 1543. It is believed that Titian undertook the illustrations, and it is cer-

tain that Titian's pupil, Jean Calcar, executed the larger number of them.

Passing by without comment many works on anatomy, more or less artistic, which were composed after the example of Vesalius, I come to the masterpiece of the 17th century.

The finest work of art in the shape of anatomical copperplate engraving ever produced is the superb folio of Godfrey Bidloo, published at Amsterdam in 1685. The drawings were made for him by the artist Gerard de Lairesse, and the engraving is worthy of the artist and the professor. Lairesse confined himself to a close delineation of his subject, but a little of the old-time fancifulness is occasionally apparent, especially in two plates which are introductions to the osteology. In one of these a superb skeleton has just stepped out of a massive sarcophagus and holds aloft an hourglass with the air of one who discourses on the brevity of life. In the second plate the grim moralist is returning to his grave with a sardonic smile, as if he preferred his peaceful rest to the turbulent outside.

And here is a convenient place to draw attention to the humorous displays of the early anatomists. Representations of the bony emblem of mortality in fantastic positions or relations appear in quite ancient literature. The presence of such a lugubrious guest at a feast or in places devoted to pleasure is a curious usage derived, according to Herodotus, from the Egyptians. The expression "a skeleton at the feast" is proverbial, but it is inaccurate as regards its source. The word *skeletos*, in its original usage, meant a dead body—a dried-up corpse, a mummy—and it is most likely that at the feasts of the ancient Egyptians a mummy served as the *memento mori*. In the museum of the Vatican is a marble thorax which exhibits the ribs only, but very finely proportioned. It is thought that this was intended to serve a similar purpose.

There was much sardonic humor in the old anatomists. In Valverde's work is to be seen an *écorché* who holds a knife in one hand, and with the other holds up the entire

skin of his body, which he gazes upon with looks of affectionate admiration.

A skeleton moralizing, Hamlet fashion, over a skull is one of the drawings of Vesalius, who died in 1564, the very year of Shakespeare's birth.

The work of Charles Estienne, better known under his Latinized name of Stephanus, was published in 1545. It is a curious attempt to conjoin art and anatomy, generally in a fantastic manner. For example, the first of the plates illustrating the anatomy of the brain contains a kneeling figure supporting himself by a staff and obligingly presenting his cranium for inspection. The scalp, which has been removed, is hanging on the branch of a tree, and upon another tree is suspended a framed table with the necessary description. In the next plate the skull-cap is hanging on the tree, and the surface of the brain is presented to view. Each following plate has a different landscape, and as section after section of the brain has been removed, the benevolent cadaver places himself in the most convenient position for the full enjoyment of the spectacle he offers. In the last plate, when all the brain has been taken away, half kneeling, half lying, but courteous to the last, he presents the bony base of his cranium to show that all is gone. The landscape is sympathetically conceived. The shattered trunk of a tree is near the figure and a ruined castle in the background.

In Laurentius' Anatomy, published in 1595, the climax of absurdity is reached, for a professor, demonstrating the anatomy of a body which lies before him, has the entire front of his own chest removed and thrown over his right shoulder, so as to exhibit the heart and lungs for the edification of his class.

In one of the earliest illustrated books produced after the invention of printing, the famous Nuremberg Chronicle, published in 1493, there is a plate entitled *Imago Mortis*, the Image of Death. A manikin is playing on a flute while two skeletons and an *écorché* are dancing. The invention of the manikin is ascribed to Fra Bartolomeo.

This fantastic taste was common in the period of what has been called the paganism of the Italian Renaissance. Everything was paganized. Christian names were converted into Latin. God, Christ, the Virgin, the angels, were described in the writings of the day under names which had belonged to the "kind old gods" of Olympus. Sanazzaro in his poem *De partu Virginis* not only invokes the muses of Helicon to celebrate the birth of Christ, but introduces the old pagan river-god Proteus, who announces the forthcoming event to the river-god of Jordan.

In the quaint compositions known as the Dance of Death this peculiar humor, always associated with the moral lesson of the uncertainty of life, is curiously exhibited. There is a charming composition of Le Sueur which embodies the same moral. A young Epicurean, richly dressed, crowned with flowers, and brilliant with health, promenading through a delicious landscape, sees a tomb on which is the inscription *Ipse Epicurus obiit*—even Epicurus died.

The attempt to teach artistic anatomy by models began at a very early period. The most usual form was a figure from which the entire skin and underlying cellular tissue had been removed, so as to exhibit all the superficial muscles of the body. It was carved in marble or molded in plaster. We have no English word for it except the clumsy term of "flayed figure," so we still use the French word *écorché.* These figures generally suggest the flaccid muscles of the dissecting table. *Écorchés* were made by Bandinelli, a contemporary of Michelangelo, by Tortébat, Bouchardon, Houdon, Salvage, and others. The figure most commonly seen in studios is the composition of Houdon. Alphonse Lami, a French statuary, about forty years since produced a new and very admirable one. It was a life-size figure known as *Le bêcheur*, and represented a man digging with a long-handled spade. Lami's theory was to represent "a living man without his skin in a given action." Another very excellent *écorché* was made later by a young artist named Eugène Caudron.

Attempts have not been wanting to improve the melancholy lay-figure of the studio. An ingenious method has lately been put forth by Dr. Eliza Mosher, which she calls a "posture model." The vertebræ of an articulated skeleton are strung upon a lead pipe, through which runs a copper wire, to modify its flexibility. The spinal column thus prepared can be curved to represent any position of the body desired.

It is told of a famous artist that upon entering his studio and finding his pupils busily engaged in drawing from an *écorché*, he flung the plaster cast into the fireplace, saying, "If you want to paint muscles, look at the living figure with the skin on!" And yet the living model has its disappointments. How little there is of the heroic in the tired arm of the supposed warrior! Sir Charles Bell said that ropes are wanted to hold the model up while the picture is completed, and in Cheselden's Anatomy there is a plate of an *écorché* tied with ropes to the branches of a tree, to keep him in the position needed to show his muscles.

A subject in which anatomy is, to some extent, concerned is the much disputed one of idealism and transcendentalism in art. Cicero said that more beautiful images could be conceived in the mind than are seen by the eye, and a modern poet, in like vein, says:

> "No true painter ever set on canvas
> *All* the glorious visions he conceived."

But no painter's pencil or poet's thought can do more than combine the visible and the remembered. The poet invented and the artist painted centaurs, fauns, satyrs, mermaids, and other fantastic creatures, but they were only combinations of the human with animal form. Marvelous skill was shown in giving the expression of the animal to the partially human face. It is not the pointed ears, the budding horns, and goat's legs alone which make the sylvan deity, but the leering sensuality of the goatish face, inimitable as it seems to be by modern art, is the characteristic of Pan and his cohort.

Sir Joshua Reynolds, in a strong passage denying the possibility of anything superhuman in art, ends thus: "We are forced to confine our conceptions, even of heaven itself and its inhabitants, to what we see in this world." He might have added that the inventions of Christian art may take rank in their absurdity with the grotesque monsters of classic lore.

Ideal beauty on theoretic grounds reminds one of the endless geometric schemes of human proportion. They produce an exact and symmetrical result, but as lifeless and uninspiring as a composite photograph. The Grecian sculptor, ignorant of dissection, was master of the anatomy of form. He sought to attain the highest ideal beauty not by any fanciful theory, but by selecting the most perfect type of every feature or limb.

The story may not be true that "the statue which enchants the world" was really composed from "the mingled beauties of exulting Greece," but the principle involved is unquestionably correct. The sculptor Rinehart stated to a friend that his Clytie was the result of a study of twenty of the finest models in Rome.

It is told of a young painter that before beginning to work he knelt down and prayed to be delivered from his model. His aim was to idealize, not to copy. And with the highest success of the artist in embodying his ideal comes to the beholder, after long gazing, an uneasy feeling of desire for something still loftier. "The loveliest Madonnas of Christian art," said Ruskin, "fall short of their due power if they do not make their beholders sick at heart to see the living Virgin."

There are some famous pictures which represent the teaching of anatomy, and in these the obligations of anatomy and art may be fairly considered to be reciprocal.

Few persons can have visited The Hague without seeing and admiring Rembrandt's painting of "The lesson of anatomy." It represents Professor Tulp, the friend and protector of the artist, demonstrating the muscles of the forearm before

his class. The bright, intelligent faces of his auditors, the easy grace of the master, who with one hand is lifting the tendons and with the other is impressing upon his hearers the functions of the muscles, is a masterpiece of expression. Tulp's name is hardly known now except to the student, but he will live in history as long as Rembrandt's painting shall endure. It is not generally known that there is in existence a still finer painting by Rembrandt of a like subject. The Tulp picture was produced when the artist was twenty years old. In his fiftieth year he painted a Lesson of Anatomy as given by Professor Deyman. This was lost sight of for a very long period, and it was only recently that it was discovered in London and secured for the Royal Museum of Amsterdam. Unfortunately a large part of the painting had been destroyed by fire, and of the class only one figure remains, that of a youth with a fine face, dressed in the sombre garment set off by a long white collar characteristic of the period. He holds the skull-cap of the subject in his hand. Of the professor nothing is left but the hands, which are extremely beautiful, and which hold two instruments in true surgical style, with which the membranes of the brain are being demonstrated. The cadaver is in a half sitting position for the convenience of the lecturer, and there is in the face and attitude something of that grim half-consciousness which the older anatomists delighted to represent. The body is foreshortened with daring skill, and what is left of this wonderful painting shows the great advancement which time had brought to the skill of the master. This fragmentary painting was discovered by Volkmaier, the biographer of the artist, who knew of its existence from Rembrandt's papers. I have seen only one notice of it in print.*

In the Royal Museum of Amsterdam a room has been dedicated to a collection of paintings of "Lessons of Anatomy," by Dutch artists, extending from 1603 to 1758. This

* Paul Triaire. Les leçons d'anatomie et les peintres hollandais aux xvi^{e} et xvii^{e} siècles, [Paris] 1881.

fragment of Rembrandt's is the especial treasure of the collection. There are in all nine of these paintings.

After this partial retrospect of the progress of artistic anatomy, and of the methods by which it has been taught, our first question occurs—Did anatomy aid early art?

Early Greek art was most likely prehistoric. It has been surmised, and the surmise repeated as if it were a settled discovery, that it was during the Homeric period that the Venus of Melos, of the Medici, the Laocoon, the Niobe, and other masterpieces were produced. All this is very uncertain and very unimportant. It is sufficient for the present purpose to know that the age of Pheidias long preceded the first crude attempts to study human anatomy on dogs, pigs, and monkeys. If by anatomy be meant dissection, the question then answers itself. External form was seen in its perfection in the sunny climate of Greece. The open-air life, the free garments, the games of the arena, all tended to this end. Other things conspired to make the age of Pheidias especially favorable for the development of art. The gorgeous spoils of the defeated Persians supplied stores of gold and ivory for the chryselephantine statues, and the continued tribute for defense from the Grecian States under the rigorous rule of Pericles furnished ample resources for architectural and artistic work. Athens had been destroyed, and new temples and buildings were to be erected. These demanded the highest efforts of plastic art, whether in full or in semi-relief, for their adornment. It must not be supposed that Pheidias trusted only to his marvelous genius. He was early trained in the school of Hegias, and later with his famous fellow-pupils, Myron and Polykleitos, in the school of the Argive sculptor, Ageladas.

Another potent cause for the supremacy of art in that time was the intense love of beauty inherent in the Greek. "I take the gods to witness," are the words put in the mouth of a Grecian youth, "that I would rather have a fair body than a kingly crown."

It is to be observed that the male figure predominates in

Greek art. Winckelmann, of whom Madame de Staël said that he made himself pagan for the purpose of penetrating antiquity, says: "As it is confessedly the beauty of man which is to be conceived under the general idea, so I have observed that those who are observant of beauty only in women, and are moved little or not at all by the beauty of men, seldom have an impartial, vital, inborn instinct for beauty in art. To such persons the beauty of Greek art will ever seem wanting because its supreme beauty is rather male than female."

Winckelmann does not enter into the reasons for this preference for masculine figures, but the opportunity for superior development of the muscles, especially in martial or athletic postures, naturally explains it. The beauty of goddess or nymph in the marble fills the mind with admiration, but it is of a calm and placid nature. "Greek sensuousness," says Pater, "does not fever the blood. It is shameless and childlike." To the Greek artist external form, in motion or in repose, was all the anatomy he could acquire or need. He found that "acuteness and fidelity of eye and obedience of hand brought precision; precision, proportion; proportion, beauty." *

How many theories upon the nature of the beautiful have been written! From Longinus to Ruskin and Spencer, where are now their followers? Who does not feel forced to say with Omar Khayyám:

> "I have heard great argument
> About it and about; but evermore
> Came out by the same door where in I went."

There are certain qualities which, from the anatomist's point of view, are essential to beauty of form. The skeleton, the foundation of the figure, though unseen, must be perfect, the long bones arched, and the joints not clumsy. The muscular system must be well developed in both sexes. Fat in moderation contributes to the roundness of outline, though its excess is fatal to ideal beauty. Last of all, the

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* Fuseli.

skin, with its concomitants of color and hair, becomes the quintessence of form in its wonderful capacity of expression. A hundred years ago Diderot, the ablest art critic of his century, said: "A thousand painters have died without having comprehended flesh; a thousand others will die without comprehending it."

The glorious period of Grecian art is, to us, emphatically the age of sculpture. That it was equally famous for its paintings we know from history, but they have all long since perished. A beautiful specimen of what is thought to be a painting of those early days was discovered at Cortona. The peasant who dug it up had used the slab as a door to his oven. The painting consisted of an exquisite female face of the purest Grecian type. An enthusiastic critic, after dilating on its beauties, says: "It is a pagan face, and the Christian soul has never dawned in those eyes." What the particular expression of the "Christian soul dawning in the eyes" may be he prudently leaves unexplained. It was a touch beyond the reach of common sense.

Leaving the consideration of the period of the masterpieces of Grecian sculpture, let us briefly consider what anatomy did for art at the time of the Italian Renaissance. Four names present themselves especially in this connection.

Luca Signorelli (1442–1524) exhibited a remarkable knowledge of anatomy, from which it is known that Michelangelo greatly profited. Luca painted a noble picture representing a man carrying a dead youth on his shoulders. Both figures were nude, and it is probable that the painting was made in a lazar-house, for at that time the hospital, the gallows, and the churchyard were the only schools of anatomy, or it may have had some allusion to a touching incident of Luca's life. He had a favorite son, an extremely beautiful youth, who was one day brought home dead, having been slain in a duel. Luca carried him into his studio, removed the clothing, and tenderly washed the blood stains from the beautiful body. He arranged the light, and, with dry eyes and firm lips, remained hour after hour painting on his can-

vas the lovely youth, so ruthlessly torn away, before decay should touch him. There is no doubt of the authenticity of this story, but it is remarkable that in a Latin poem by Vincent Bourne a like action is attributed to Apelles. It is probable, however, that the name is used generically for a painter, and that the incident referred to is the one just related. Cowper has translated the poem. One knows not which to marvel at the most, the devotion of the artist or the stoicism of the father.

The exquisite taste of Leonardo da Vinci, the great predecessor of Michelangelo and Raphael, saved him from any obtrusive display of his knowledge of anatomy, which was, perhaps, unequalled in his day. The anatomical sketches by this master still preserved at Venice are masterly drawings of great accuracy.

How Michelangelo obtained the knowledge of anatomy which he employed with such wonderful effect we do not know. His sketches of the partially dissected body and of the skeleton are often incorrect, and it is probable that he trusted to his memory of what he had seen in the hospitals or graveyards. He rarely introduces any portion of landscape. The human form was all to him. In one of his sonnets to Victoria Colonna he says:

> "Nor hath God deigned to show himself elsewhere
> More clearly than in human forms sublime;
> Which, since they image him, compels my love."
> (Symonds' translation.)

It is an old saying "*Ars est celare artem*," It is art to conceal art. Anatomy should not be learnt to parade it. The muscles in some of Michelangelo's figures are so enormously developed as to seem distorted. Quatremère de Quincy says of him that while he made his figures show the very life of motion as no other artist ever has done, none of them seem to be capable of thought or feeling. Physical strength in perfection in every posture there was, but a sombre expression of face characterized them all. While there is some ground for this criticism, it is too general when applied to

all the works of this mightiest of the great masters. A fine illustration to the contrary is to be found in his famous Pietà. There is the Madonna with the dead Christ on her knees. Her small head, youthful face, large torso, and ample limbs are illustrations of Michelangelo's conception of the heroic. The pathetic stillness and beauty of the relaxed dead body and the contrast to the vigorous form of the mother, with unspeakable woe in her face, is beyond description.

It would be a juster criticism to say that he was wanting in the especial characteristics of the art of Pheidias, simplicity and grace, or as Winckelmann admirably termed it, " noble *naiveté* and placid grandeur."

Probably the most striking example of Michelangelo's knowledge of anatomy was displayed in the famous cartoon which he produced in rivalry with Leonardo da Vinci. He selected as its subject an incident of the war between the Florentines and the Pisans. A number of Florentine soldiers were bathing in the Arno when an alarm was sounded betokening the approach of the enemy. The men are seen rushing from the river to the bank and making desperate efforts to put on their clothing and armor. The muscles of the warriors are superbly displayed and are free from the exaggeration which is seen in some of Michelangelo's colossal figures. The cartoon perished, but a copy fortunately survives of what, perhaps, was only a portion of the original drawing.

The anatomical sketches of Raphael which are yet in existence show mostly, as might be expected, a masterly knowledge of external form. There is little of the robust musculature of his great contemporary in his works, but it is generally correct.

As we leave the fascinating period of the Renaissance we come to the time when it was no longer necessary to study anatomy by stealth at the gallows foot or in the lazar-house. Time will not permit of an attempt to trace the effect of such studies further. Brief and discursive as this sketch has been, I shall venture to suggest certain conclusions from it.

That neither in ancient Egypt, the Orient, nor in ancient Greece did art obtain any assistance from human anatomy by dissection. That nevertheless the finest examples of sculpture of the human figure that the world has ever seen were produced in ancient Greece.

That not until about the sixteenth century were such studies made possible, and that not until the time of Vesalius was there any thorough knowledge of human anatomy.

That an acquaintance with muscular anatomy by dissection, though to be recommended as useful, forms but a comparatively unimportant part of artistic anatomy. That this latter must be the study of the external form of the living body in repose or in motion, with a profound observance of the modifications produced by sex, age, and race.

That, in short, physiology, and to some extent external pathology, meaning by this latter term the effect of disease, wounds, and death upon the body, with a certain amount of study of the muscles by dissection, make up what should be understood by artistic anatomy, or, as I should prefer to term it, artistic morphology.

OBITUARY NOTICES.

ASA OWEN ALDIS.

[Read before the Society, May 26, 1894.]

Asa Owen Aldis was born at St. Albans, Vermont, in 1808, being the son of the principal lawyer of that town, who settled there, having removed from Franklin, Massachusetts, a few years before his son's birth.

Judge Aldis used to relate how his father's selection of St. Albans was made on a comparison of the trial lists of the courts of that county with those of Pittsburg, Pennsylvania, the other place which he thought of selecting for residence. Finding that the average number of cases on trial at St. Albans was greater than that at Pittsburg, his father selected the former place. His father had been left an orphan at tender years, in charge of an English officer, at the outbreak of the Revolution. When the war began the officer went back to England, and during the whole of the Revolution the young lad was ignorant of the condition of his property; but at the close of the war the officer showed that he was absolutely faithful to his trust, and turned over the property not only intact but increased. It thus resulted that both the father and the son were in possession of sufficient means for the maintenance of life according to the standard of gentlemen of their day.

The first fact which Judge Aldis remembered with distinctness was the sound of the firing of cannon at the battle of Plattsburg, on the 11th of September, 1814. He was graduated at the University of Vermont, at Burlington, and afterwards studied law at Harvard College, where he was a friend of Professor Greenleaf, and also of Judge Story.

He developed and maintained altogether the most profitable and important practice in Franklin county, Vermont, after his admission to the bar, until 1858, when he was appointed associate justice of the supreme court of Vermont. Owing to the serious illness of one of his daughters, he determined to remove to Nice, France. By the merest accident he received the appointment of United States consul at that place through the intervention of Senator Edmunds, who had been a student in his office. His term of office was continued from 1865 to 1870, to the entire satisfaction of American travelers.

He returned to the United States and, visiting Washington with the idea of removing there, was offered on the same day an appointment to either one of two positions—that of chairman of the Southern Claims Commission or member of the commission for adjusting claims under the treaty with Mexico. He accepted the former position and discharged his duties from 1871 until the expiration of the commission, when he was immediately appointed a member of the commission for the adjustment of the French claims. After the expiration of the last commission he held no official position, but continued to reside in Washington, to the gratification of his friends, until his death.

His reported opinions bear evidence of much intelligence and training and ability as a lawyer, as his work on the various commissions in Washington showed conscientious, painstaking labor, which was united to the mental characteristics which had made him distinguished as a judge and lawyer in Vermont. His bright conversational qualities; his genial, sympathetic disposition; his fixed friendship; his love for the beautiful in nature, literature, and art were the facts of his lifetime best known to his friends. As an illustration of the joyousness of his nature, a friend relates that Judge Aldis, on a lovely afternoon in the early summer, said to him in Washington, with the fervor of a natural young girl, "Oh! my friend, what a beautiful city we live in. I cannot tell you how beautiful it seems to me."

Although at that time doubtless over 80 years of age, his heart was young and impressionable.

An example of such a life lightens very materially the burdens imposed by nature on man.

His death occurred on the 24th of June, 1891.

Jos. K. McCammon.

ROBERT STANTON AVERY.

[Read before the Society, February 16, 1895.]

The subject of this sketch was born May 1, 1808, on a farm near Preston, New London county, Connecticut, and died, in his 87th year, of paralysis, on September 12, 1894, in Washington, D. C., his remains being interred beside those of his wife, at his birthplace. He was named after his father, Robert Stanton Avery, who married Miss Sarah Crary, June 14, 1807, and was the eldest of their family of eight children. He traced his descent from Christopher Avery, who was born in England in 1590 and died in New London, Connecticut, in 1679. This ancestor emigrated to the new world, accompanied by his son James (1620–1700), landing at Salem, Massachusetts, about the year 1630, or only ten years after the first settlement at Plymouth. The grandson of Christopher was also called James (1646–1748), and the great-grandson was another Christopher Avery (1679–1753). The second Christopher's son (1705–1790) and grandson (1732–1794) were both called John, and his great-grandson was Robert Stanton Avery (1771–1846), who was the father of the Avery whose biography is briefly outlined here. The remarkable longevity of his ancestors, as indicated by the above dates, is worthy of note.

Robert S. Avery, Junior, acquired his early education in the district school near his birthplace. An attack of measles in his 13th year left him with dyspeptic tendencies, which caused him much suffering at times throughout his life.

He very early became fond of books, reading whatever he could obtain. At the age of 17 he commenced teaching school, and for three or four years taught in the winter and worked on the farm in the summer. In his 21st year his mother died, and shortly afterwards he left home to complete his education, going successively to academies in Plainfield, Connecticut, Whitesboro, near Utica, New York, and Troy, New York. His father was unable to provide sufficient funds for his instruction, so he went where he could assist in teaching the younger scholars in return for his tuition. He also picked up any jobs of work he could, and even cultivated some rented land near by, to further increase his limited resources. Although thus very seriously hampered, he studied so diligently that his progress was rapid, especially in the mathematical and physical sciences and in Latin and Greek.

In 1833 he started a private school in Fall River, Massachusetts, and attended the lectures of Sylvester Graham on anatomy, physiology, and hygiene, which made such a strong impression upon him that he began a series of experiments upon his diet, which happily resulted in strengthening his digestion and at the same time diminished the cost of his living. As he was unable to get scholars enough to make his school pay sufficiently well, he became teacher for a term each in various towns in Connecticut and Massachusetts, and even tried working as a traveling salesman for a Boston publishing company. After a time he found an opportunity to learn the trade of cabinet-making in Worcester, Massachusetts, where he worked at the bench for three years. As his manual work did not take all his time, he studied the French language by himself, which he readily acquired because of his knowledge of Latin.

About this time he intended to become a physician, and proceeded to Philadelphia to enter the medical college there; but it was necessary for him to find some employment that would supply money enough for his medical course and at the same time afford sufficient leisure to permit his attend-

ance upon the lectures. This was in the spring of 1837, when all business seemed paralyzed, and, failing to secure work, he was obliged to abandon the idea of studying medicine. He had so little money that he was unable to ride in the stage coaches, and walked all the way into Ohio, passing through Pittsburg and Wheeling. He taught school for one term in Ohio, and then went into Kentucky, where for five or six years he taught school in log cabins in the woods near Louisville. During these years he frequently met with scholarly men in Louisville, and association with them kindled a longing desire for a higher education, which finally induced him to give up teaching in order to become a student once more, although then over 35 years of age. He thought of going to some German university, but visited Cambridge before deciding, and was so well suited with the facilities afforded by Harvard College that he decided to enter its Divinity School. The principal reason for selecting this department of the university was a financial one, the theological students being under less expense for tuition and board than the others. A large part of his time at Cambridge was spent in studying the Scriptures critically in Greek and Hebrew. He also read some German and a great deal of theological Latin and attended many of the university lectures on languages, literature, history, and the sciences. He graduated in July, 1846, obtaining a license to preach, in accordance with the custom of the Unitarian denomination. Hard study and lack of exercise had impaired his health; his eyesight was weak and his voice became too feeble to be heard well in large assemblies, so that after occasionally occupying vacant pulpits in Massachusetts and neighboring States without securing an appointment he returned to Preston, not thinking of giving up the profession, but hoping to regain sufficient health to become able to pursue it successfully.

The outdoor life of the farm soon restored him to good health, but he was destined never to resume preaching again. His father having died a few days before his gradua-

tion, he was called to settle up the estate, and for seven years he worked on the farm, taught school during the fall, and continued his studies in the languages and higher mathematics and their application to the physical sciences, particularly to light. During these years he made some scientific investigations, and constructed a reflecting telescope over five feet long. The speculum metal for the mirror was made and cast by him and then ground into shape and polished with such success that the instrument had a very fair definition. He made an equatorial mounting, with graduated arcs, which enabled him to roughly estimate angular distances. In the course of time the mirror became dull and covered with dust from lack of use, but he retained the instrument to the close of his life and often spoke of it as having given him much pleasure and instruction.

In October, 1853, he accepted a position on the United States Coast Survey. He came well prepared for his new duties, and it was not long before he attracted the attention of the superintendent, Prof. A. D. Bache, who selected him as special computer to carry out his plans for the reduction and prediction of tides. Some of these calculations were long and tedious, requiring many months of hard work. For instance, on one occasion he had more than 700 conditional equations, each containing 23 unknown quantities, for which he had to obtain the most probable values by the method of least squares. He computed the occultations of the larger stars of the Pleiades, the principal object being the correction of the tables of the moon's motions and the improvement of terrestrial longitudes. He was promoted from time to time as he proved his fitness, until after twelve years of work he was made chief of the tidal division of the Survey. He held that position for more than twenty years, when, owing to failing health and advancing years, at the age of 77 he resigned, on September 30, 1885, having spent 32 consecutive years in the service. He was a painstaking and accurate computer, with good administrative abilities, and was especially successful in directing the labors of others,

for during more than a score of years he never had a case of insubordination or personal altercation. He collected the scattered results of the various reductions for nearly a thousand series of tidal observations into a single register, which has proved of great value to his successors. He developed a number of new methods for discussing tides of different types, most of which were never published, and his inventive ability enabled him to greatly improve the automatic tide gauges used to secure continuous records of the tides. At the time he was made chief of division the Coast Survey had published only some general tables giving the elements for predicting tides at various places. He at once urged the preparation and annual publication of the predicted tides a year in advance for the benefit of mariners. A volume of tide tables for the Atlantic coast of the United States for the year 1867 and another one for the Pacific coast for the same year were printed in 1866, only a year after he assumed charge of the tidal work. These tide tables gave the time and height of each high tide throughout the year 1867 at all the principal ports of our eastern coast, and the time and height of both high and low tide was given for the western coast. They were the first tide tables of the kind ever issued by our Government and have been published regularly every year since then down to the present (1895), although somewhat enlarged of late years, and the predictions for the Atlantic coast also made to include the low as well as the high waters. He had the honor of being in charge of the tidal division when the harmonic analysis for tides was first introduced into this country. This method of treating tides seems to be chiefly due to Sir William Thomson, now Lord Kelvin, although Laplace had previously discussed some of its principles. Prof. William Ferrel in this country and Prof. George H. Darwin in England made important modifications and brought it to the present degree of perfection, which renders it the most beautiful and satisfactory system for the analysis and prediction of tides ever invented. Before the invention of tide-predict-

ing machines it was practically impossible to employ the harmonic tidal constants for prediction, owing to the amount of labor involved; but the construction by the Survey of Ferrel's tide machine rendered it easy to predict from harmonic constants, and therefore stimulated the introduction of the harmonic analysis. His official writings are contained in the annual reports of the Coast Survey.

Mr. Avery was slightly above the medium height and of that rough, rugged appearance often seen among the hardy sons of New England. He was of a very quiet, modest, and unassuming manner, somewhat shy of strangers, yet very pleasant and companionable to those who were intimate with him. He was a man of most amiable and sweet temper, for, during more than twenty years of intimate association with him, both officially and in his home, the writer never witnessed any manifestation of anger or resentment, nor even the appearance of being provoked over any of the many petty occurrences that usually exasperate less happily endowed persons. He was just and kind in his estimation of his associates, and was an upright man of the strictest integrity in all his dealings. He was very simple in his wants and not fond of display, so that during the third of a century he was employed by the Government he saved a large portion of his income, thus obtaining capital which, by fortunate investments, furnished the means for comfortable support after his withdrawal from official life. Although educated for the Unitarian ministry, he was too liberal in his religious views for the majority of the adherents of even that broad creed. In later years he ceased believing in any form of Christianity and professed to be a pantheist.

He remained unmarried so long that it was generally supposed by his friends he would always remain single, but at the age of 53 he married Miss Lydia Tyler Meech, on October 16, 1861. They had no children, but she was loving and devoted to his interests, and they lived happily together until her death, on November 18, 1890. She was a daughter

of the Rev. Levi Meech, of North Stonington, Connecticut. After her death he had his niece, Julia N. Avery, a daughter of his deceased brother Isaac, to keep house for him. She gave loving attendance to every wish of the now aged and infirm man, as though he had been her own father; indeed, very few parents have children as untiring and unselfish in their devotion to them as Miss Avery was to her uncle.

After his retirement from the Government service he was interested in preparing for the press a set of mathematical tables; also a series of school books designed to extend the teaching and use of phonetic spelling, a few of which were published at his own expense during the last year of his life, in which he makes a very strong plea in favor of spelling our language according to sound and extends the alphabet so as to make it possible.

Mr. Avery was a member of the American Association for the Advancement of Science; of the Philosophical Society of Washington and its Mathematical Section; of the Anthropological Society of Washington; of the National Geographic Society; of the American Association of Inventors and Manufacturers; of the Astronomical Society of the Pacific Coast; of the American Society for the Extension of University Teaching, and of the American Academy of Political and Social Science.

He will be chiefly remembered because of his magnificent bequest to the Smithsonian Institution, the amount of which cannot be accurately evaluated before the estate is settled, but which is generally estimated to be worth anywhere from fifty to more than one hundred thousand dollars. In his will, dated July 22, 1893, he appoints his niece, Julia N. Avery, executrix of the estate, and, after bequeathing small sums to various relations with the express condition that the legatees acquiesce in his will or forfeit their share entirely, he directs that the remainder of his estate shall go to the Smithsonian Institution and its successors forever. He suggests that the bequest shall be called "the Avery fund"

or "the fund contributed by Robert S. Avery and his wife, Lydia T. Avery, for the extension of the sciences." He desires that the income from the fund be used for promoting publications relating to phonetic type and printing; for publishing lectures and treatises upon the laws governing an ethereal medium, and for prizes to be given for essays on the phenomena of electricity, magnetism, light, and heat.

LELAND P. SHIDY.

GEORGE BANCROFT.

[Read before the Society, October 15, 1892.]

GEORGE BANCROFT became a member of this Society in the year 1875. Although he never took an active part in its proceedings, he remained a member until his death, which occurred January 17, 1891, after an illness of two days, the result of a cold.

Mr. Bancroft was one of the most eminent of American authors, scholars, and citizens.

Though preëminently an historian, his career covered a wide range of occupations. During his long life of ninety years he was student, poet, tutor, preacher, school-teacher, politician, office-holder, orator, historian, cabinet minister, minister plenipotentiary, diplomatist, and in his later years he achieved success and no little fame as an amateur florist, as if he had come to believe, with Voltaire in his "Candide," that the noblest occupation of man is to cultivate a garden. Mr. Bancroft's roses were the delight of his old age and the admiration of all who saw him in his home.

George Bancroft was born in Worcester, Massachusetts, October 3, 1800. His first American ancestor in the Bancroft line was John Bancroft, who came from England to New England in 1632. His father, Rev. Aaron Bancroft, D. D., while a mere boy, took part in the early battles of the Revolution. He afterwards became a Unitarian minister

and was settled in Worcester, Massachusetts, for the long period of fifty years. Of Mr. Bancroft's American ancestors his father was the first who gave evidence of scholarly and literary tastes. He wrote a "Life of Washington," which was reprinted in England. He was a member of the American Academy of Arts and Sciences and was also president of the American Unitarian Association.

George Bancroft entered Harvard College in his thirteenth year and graduated before he was seventeen. He was then sent to Germany, on the recommendation of the faculty, to pursue a course of study with the view of becoming a teacher in the college. His degree of Doctor of Philosophy he received from the University of Göttingen in 1821, and he afterwards studied at Berlin and Heidelberg. Despite his youth, he seems to have won acquaintance with distinguished scholars, for, as a student and later as an European traveler, he met Schleiermacher, Humboldt, Goethe, Cousin, Constant, Manzoni, and Bunsen.

Returning home in 1822, he entered upon duty as a tutor of Greek at Harvard College. With this work, however, he soon became discontented, owing, it is understood, to dissatisfaction with the college authorities, who failed to afford him the encouragement he sought in certain special plans of work. In 1823, with Professor Joseph G. Griswold, then librarian of the college, he left Harvard, and together they set about establishing what seems to have been to them a kind of ideal school for boys.

Mr. Bancroft had inherited not only his father's scholarly and literary tastes, but some of his theological bias also, and while a tutor at Cambridge he preached several sermons. He also attempted poetry, with greater success he seems to have thought, since he soon abandoned preaching, but he did not forsake the muse until he had published a volume of eighty pages entitled "Poems by George Bancroft."

The school founded by Mr. Bancroft and his friend Griswold was known as the Round Hill School, and became quite famous. Beginning with a few dozen pupils, the school

increased until the attendance reached several hundred. The teachers secured a beautiful estate near Northampton, Massachusetts, where they established a delightful students' home, in which study was combined with healthful bucolic occupations and amusements. Farm life and long rambles were combined with thorough mental training. Pupils came from all parts of the country, many of them from the Southern States, and many men of note there obtained their early education. Though an excellent school, it was not financially profitable to its founders, and in 1830 Mr. Bancroft abandoned the enterprise.

During his stay at Round Hill Mr. Bancroft, always an industrious student, did much literary work. He contributed to the North American Review, and translated Heeren's "Politics of Ancient Greece" and "Jacob's Latin Reader." He also delivered an oration in Boston advocating universal suffrage and Democracy.

After leaving Round Hill he was elected, without his knowledge, to the State legislature, but declined the position. He also declined a nomination to the State senate. In 1831 he published a paper on "The Bank of the United States" and another on "The Documentary History of the Revolution." He likewise prepared at this time the address to the people of Massachusetts for the young men's Democratic convention. He was in politics a Democrat, and always possessed in a high degree that democratic instinct which De Tocqueville emphasizes as the foremost characteristic of the American citizen.

In 1834 appeared the first volume of his great work, "The History of the United States." In 1835 he took up his residence at Springfield, Massachusetts. The second volume of his history appeared in 1837 and the third in 1840. From 1838 to 1841 he was collector of the port of Boston, having been appointed to that position by President Van Buren.

In 1844 he was the Democratic candidate for governor of Massachusetts, but was defeated, though he received a large vote.

In 1845 he was appointed Secretary of the Navy by President Polk, and in that office performed valuable service. He established the Naval Academy at Annapolis, entering into the project with great zeal and intelligence. In fact, in all his executive positions he achieved success. He also rendered important service to the Naval Observatory in Washington by obtaining increased appropriations and securing professors of great ability.

In 1846 he was appointed minister plenipotentiary to Great Britain, where he remained until 1849. During his residence in England he continued his historical researches and worked upon his history.

On returning from Great Britain he took up his residence in New York, where his friend Griswold, who had meanwhile abandoned the school at Round Hill, was now superintendent of the Astor library, and from him he doubtless received great assistance and encouragement.

From 1849 to 1866 he seems to have devoted himself wholly to historical work.

In 1866 he was invited by Congress to pronounce a eulogy upon Abraham Lincoln, and in 1867 he returned to public life, having been appointed by President Johnson minister to Russia.

Though a lifelong Democrat, taking no part in the anti-slavery agitation, and even as a cabinet minister making no resistance to the encroachments of the South in the interest of slavery, yet, in view of his great ability, his acquaintance with foreign governments, his experience, and his skill as a diplomatist, he was selected by General Grant as a fitting representative of the United States to a foreign nation, and, in 1871, was accredited minister to the German Empire.

While at Berlin he rendered important service in settling with Great Britain the dispute respecting the northwestern boundary of the United States, and when the question was submitted to the King of Prussia Mr. Bancroft prepared the argument for this Government. He also prepared the treaty

with Prussia in which that government recognized the right of emigrants to the United States to throw off allegiance to their mother country.

Mr. Bancroft was recalled from Germany, upon his own request, in 1874, and upon returning to this country he established his winter residence in Washington and his summer residence in Newport, Rhode Island. In the same year he completed the tenth volume of his history, and in 1876 he published a revised edition of the work in six volumes. He subsequently added two volumes on "The Formation of the Constitution."

His great work, as completed, covers the period of our history from the discovery of America by Columbus to the inauguration of President Washington.

Mr. Bancroft was undoubtedly fully entitled to the commendation and great fame which he achieved by reason of his historical work. As a student his researches were exhaustive, and as a writer he was both graphic and picturesque. He was not only an industrious student and an able writer, but he was a philosopher and statesman as well, and his work abounds with maxims lucidly stated and of the profoundest import.

Later critics have pointed out what seem to them notable defects in his work. They have maintained that his graphic style was secured at the expense of accuracy; that his quotations were sometimes paraphrases rather than exact transcripts; that he gave undue prominence to rumors and manuscript records, and that in his revisions he failed to profit by the researches of later historical students. They have even gone so far as to forbode from these causes a future diminution of his fame; but I do not find that any one has predicted a more satisfactory performance of the work, as a whole, than he has achieved.

In 1886 Mr. Bancroft published a monograph entitled, "A plea for the Constitution wounded in the house of its friends," in which he protested, in an elaborate disquisition on the Constitution of the United States, against the opinion

of the Supreme Court—Mr. Justice Field alone dissenting—wherein the Court asserted the right of the Government to make its notes a legal tender in payment of private debts· "This opinion," Mr. Bancroft declared, "if it should be accepted as a law, would be a death blow to the Constitution."

His last published address was delivered in the same year before the American Historical Association of which he was president.

The seventeen years which Mr. Bancroft spent in Washington and Newport were the ideal years of a scholar and a distinguished citizen. Always a worker, he still found abundant time in later life for social intercourse. He rose early and kept his secretary always diligently employed, but his afternoons and evenings were given up to his friends and visitors, and to his flowers and daily exercise. Foreign visitors to this country and members of the foreign legations were frequent callers at his house, and he had cordial relations with successive administrations.

In personal appearance Mr. Bancroft was slender and of medium height. In his later years he had abundant white hair and a long white beard, which gave him the appearance of greater height than he possessed.

His figure was familiar to many in Washington, as he appeared almost daily in the pleasant weather upon horseback, erect and handsome as a knight of old.

Few great Americans have lived through such an ideal old age.

Many of our eminent scholars and statesmen have spent their later years struggling with waning powers and disease and pain.

It was Mr. Bancroft's happy fortune to retain his health and the almost full possession of his powers, and to be able to enjoy life and to be a delight to his wide circle of friends to the very last.

Joseph B. Marvin.

WILLIAM FERREL.

Born January 29, 1817. Died September 18, 1891.

[Read before the Society, October 15, 1892.]

Among the many scientific men who have been drawn to Washington and have served the Government to the best of their ability no one has made for himself a more honorable record than our distinguished fellow-member, WILLIAM FERREL, whose death we were during the past year called upon to deplore. Ferrel became a member of the Philosophical Society in 1872, and his first communication to the Society, namely, "*On the Effects of Winds and Barometric Pressure on the Tides at Boston and on the Mean Level of the Sea*," illustrates the general tenor of the problems that occupied him during his whole life, namely, the mechanics of the atmosphere and the ocean. In this field of research Ferrel rapidly advanced from the general theory of tides and the general motions of ocean and air to specific phenomena, and from the study of an ideal perfect or frictionless fluid to the study of the actual air and water and the actual earth with all its irregularities. Many of our colleague's papers are to be recognized as successful efforts to solve problems that had hitherto been considered beyond our reach. So completely did he examine his field of study, so accurately did he select the important from the unimportant forces at work in nature, that in every one of the numerous results formulated by him we recognize that a distinct advance has been made from which there will be no need of retreat in future years. In the study of such complex phenomena as the motions of the ocean and air we have to consider the presence of some eight or ten factors, either one of which may become at times of preponderating importance. At some distant epoch man may be able to effect a general solution of equations that shall express the simultaneous influence of all these diverse forces, but at the present time we are not able to even write out the equations, much less to resolve them. Under these conditions the suc-

cess of the student of terrestrial physics depends upon the acumen with which he distinguishes between the important and unimportant forces that underlie a given phenomenon. After separating the essential factors from the less important, the treatment of the subject has to be pursued according to ordinary deductive methods, abundant illustrations of which are found in astronomical and molecular physics. In this latter process Ferrel was equally skillful, thorough, and patient. At the risk of lengthening my remarks to a rather greater extent than is ordinarily expected in the short biographical notices of our Society, I will take the liberty of enumerating a number of special cases wherein, I think, Ferrel showed his ability and advanced beyond other investigators.

If we take up these points chronologically we might almost begin with his boyish attempts to predict the eclipses of the sun and moon, when, without ever having seen an astronomy and having only the predicted places of the sun and moon as given in the farmers' almanacs for several successive years, he figured out the recession of the moon's node on the ecliptic, the irregularity of the moon's motion in its orbit, and the inclination of the orbit to the ecliptic, proceeding thus until finally he was able to predict the occurrence of the solar and lunar eclipses to within a few minutes of time.

(1.) Ferrel's first scientific paper, published in 1853, when he was thirty-five years of age, was based wholly upon his own studies of Newton's "Principia" and Laplace's "Mecanique Celeste," and it does not appear that up to that time he had ever had any personal intercourse with mathematical investigators of like tastes with himself. It is therefore all the more surprising that in this memoir he boldly develops a conclusion that he had conceived several years before, namely, that the sun and moon must have an effect upon the rotatory motion of the earth by reason of the friction between the tidal wave and the earth, whereby the length of the day is slowly increasing from age to age. He further-

more reasoned that if this retardation is inappreciable, then this must be due to the fact that it is counterbalanced by an acceleration of the rotatory speed due to the contraction of the earth as it slowly cools.

(2.) Ten years later Ferrel was able to show that the acceleration of the moon's mean motion, which is about 11 seconds per century, is probably due to the outstanding excess of tidal retardation just mentioned, whereby our day is shortening, thus producing an apparent acceleration of the moon's motion.

(3.) In a short paper of 1854, "On the Internal Fluidity of the Earth," Ferrel showed that we must consider the pressure quite as much as the temperature in discussing the question of the fluidity of great masses of solid matter.

(4.) In a paper on "Vision," in 1855, Ferrel showed, both by theory and experiment, that Brewster's and other theories as to the nature of the process by which the mind recognizes the direction and dimensions or shape of a given object are at fault, and that the actual phenomena of stereoscopic or binocular vision are only explicable on the principle that the mind bases its judgment as to the direction of an object by a consideration or consciousness of the relation between the location of the particular cell of the optic nerve upon which the image of that object is formed and the location of the optical center of the crystalline lens; furthermore, that the mind bases its judgment as to the distance of an object by a mental comparison of the location of the two coördinated nerve cells, one in each eye, on which the respective images of the object are formed. Ferrel established his explanation by many experiments, and his idea finds an especially severe test of its correctness by considering, first, the cases in which the mind can be deceived as to the distance of an object as seen by persons of normal vision, and, second, the cases of strabismus where the mind is not deceived, although the eyesight is abnormal.

(5.) In 1855, in a short paper on the "Variable Star Algol," Ferrel showed that the steady shortening of its

period may be due to an orbital motion by reason of which the component velocity, if directed toward the earth, has a steady acceleration or, if directed from the earth, has a steady retardation, and that we may expect the observed shortening to eventually cease and be followed by a lengthening of period. This hypothesis has been lately and independently renewed by Chandler, and has been verified by spectroscopic observations of this star.

(6.) In 1856 Ferrel communicated a memoir to Gould's Astronomical Journal on the problem of the "Tides with regard to oscillations of the second kind," a problem to which he returned in his "Tidal Researces of 1874," and again in a paper in the Philosophical Magazine of 1876, and finally again in Gould's Astronomical Journal for July, 1889, and December, 1890. In these various articles Ferrel maintains that an error of principle pervades the determination of a certain constant by Laplace, and the attainment of certain results as first promulgated by Laplace and accepted by Airy, Bowditch, Thomson, Darwin, and other mathematicians. He corrects both Laplace and Airy, and contends in effect that in an ocean of uniform depth around the earth and composed of fluid having no viscosity the semi-diurnal tidal wave may, for one critical depth only, have any amount depending upon certain initial conditions, but that for all other depths and for the case of nature, where friction exists, the initial tidal motions are gradually annulled, so that the tides observed by us are essentially those forced tides that are directly maintained by the sun and moon and the free tides depending on these latter, but not those that depend on initial conditions.

(7.) 1856.—In a so-called exposition of the phenomena of the gyroscope Ferrel works out the now well-known relations between the composition of rotations around different axes. Of course many others were at work upon that problem at that time, stimulated by the experiments of Foucault, but Ferrel's solution was entirely independent of European investigators, and was, I think, the first that was published

in this country. Having arrived at the unexpected conclusion that by overweighting the arm of his gyroscope he could cause the revolving disc to rise instead of fall, he had an apparatus constructed and experimentally proved the truth of his reasonings.

(8.) 1856.—The germs of Ferrel's numerous works on the winds of the atmosphere and on the currents of the ocean as distinguished from the tides are to be found in his popular and mathematical papers published between 1856 and 1861. The first of the new ideas which he infused into our notions of the mechanics of the atmosphere and the ocean consisted in the principle that any object moving horizontally on the earth's surface under the influence of its own inertia and weight and the centrifugal force due to the earth's rotation experiences a deflection to the right in the northern hemisphere. This deflection was rightly understood by Poisson and by Tracy, both of whom were unknown to Ferrel, and he was the first to recognize the great value of the influence of the earth's rotation as distinguished from the imperfect consideration of this subject that had been promulgated ever since the days of Hadley. Ferrel, in fact, showed that in all phenomena on a large scale this centrifugal deflection was the item of next importance to the density and weight of the air. The diurnal rotation also affects the vertical motions of bodies; but this he developed in later years. In these early publications he also considered the influence of the frictional resistance of the earth's surface to the movement of the air, which is next in importance to the effect of temperature and of rotation. Having shown the motions that must exist in the atmosphere and in the ocean, as produced by gravity and density and modified by rotation and friction, he then deduced the resulting barometric pressures (or surfaces of equal pressure) and completely explained the origin of the low barometer in the equatorial and in the polar regions, as also in the center of very extended systems of circulating winds, and the cause of the belts of high barometer under the

tropics of Cancer and Capricorn. In these works he deals principally with the general circulation of the atmosphere, which is a slow motion compared with the rapid movements within a cyclone, hurricane, or a tornado. Ferrel considers only the so-called steady motion of a fluid and not the vortex motions or surfaces of discontinuity that must attend the more rapid movements. He shows that there is a close agreement between the observed motions of the winds and clouds and the observed temperatures and pressures throughout the globe on the one hand and his own deductions from his adopted principles on the other, and that all the phenomena of rains and storms also confirm his reasoning. These views are applied by him to the ocean as well to the atmosphere. A steady, slow interchange of oceanic water goes on between the equator and the poles due to differences of density. This is by the earth's rotation converted into a westward motion near the equator and an eastward motion at high latitudes, so that the prevailing winds actually harmonize with and help to maintain this oceanic circulation, although they cannot be said to produce it. As a consequence of the oceanic circulation the surface of the ocean must be depressed in high latitudes and at the equator, and be elevated under the tropics relative to the ellipsoidal surface. In the case of enclosed oceans, like the North Atlantic, which have a slight gyratory motion with the hands of a watch, the surface of the ocean in its central region must be above its surface at the borders of the ocean.

(9.) As a departure from the usual course of his investigations, I note Ferrel's remarkably convenient formulæ for interpolation published in 1861 and 1865, and which I have had frequent occasion to use with a great saving of time and labor. Undoubtedly he was led to these labor-saving inventions by the irksomeness of his daily computations for the Nautical Almanac office.

(10.) In 1865 Ferrel was successful in inventing one of the most rapidly converging series as yet discovered for the computation of the ratio between the circumference and

diameter of a circle. This series particularly interested the late Prof. Benjamin Peirce, through whom it was presented to the National Academy of Sciences in 1865.

(11.) 1865.—As the result of a discussion of the tidal observations made at Brest, Dausy had concluded that a very small disturbance of the tides is produced by violent winds from the north or northeast and from the south. From this result Ferrel showed that the average effect of all the winds for the months or the year must be inappreciable at Brest. After correcting the observed monthly mean heights for local barometric pressure and for terms in the tidal formulæ depending upon the sun's declination and for the local wind effects, as found by Dausy, Ferrel demonstrates the existence both at Brest and at Key West of an appreciable annual fluctuation in the monthly means of the height of the sea. These stations on opposite sides of the Atlantic ocean feel the influence of a rise and fall in the surface of the sea caused by its general gyration about a central region. This gyration had been deduced as probable in Ferrel's memoir of 1861, and the present investigation established its actual existence and numerical value. The most rapid gyratory motion takes place in April, when the water is highest in the center of the ocean and lowest at Brest and Key West. The slowest gyration occurs in October or November, when the water stands a little lower in the center and higher at Brest and Key West.

(12.) 1873.—Having demonstrated the existence of an annual and possibly also a lunar monthly period in the velocity of the Gulf Stream (very much such as would now appear to result from the observations just published by Lieutenant Commander Pillsbury in the Annual Report of the Coast Survey for 1890), Ferrel, in 1873, published a further discussion of meteorological effects upon the heights of the tides with a special reference to the Boston harbor observations. He shows that an average northeast wind raises the sea-level only about three inches, and a southwest wind depresses it not quite so much. The very strongest winds

change the sea-level in Boston harbor only about one foot. The effect of barometric pressure is apparently about one-half of its theoretic value and decidedly less than at Brest, Bristol, or Liverpool, but nearly the same as at London. After correcting for the effect of the winds and the barometer, there is left the above-mentioned annual inequality in the mean sea-level which is not explained by either of these meteorological influences. Some of the outstanding discrepancies are explained in Ferrel's final report on the Boston Tides in the Coast Survey Report for 1873 as due to the fact that at Boston the rising barometer and the westerly winds with clearing weather combine together to lower the sea-level and suffice to mask the effect of the inertia of the rising or falling water.

A similar subsequent discussion of the tides in New York harbor and again of the tides in Penobscot bay, and finally of the tides of the Pacific coast, brings out the fact that the so-called retardation or age of the tide, namely, the delay in the times of the maxima of the tidal inequalities, is not, as was supposed by Whewall, simply the time required for the free tidal wave of the southern ocean to arrive successively at the ports of the North Atlantic. This retardation is rather an apparent phenomenon due to the defects of our analysis, which has assumed that the same principles apply to tides in deep and in shallow water. The same memoir also shows that the same annual inequality that holds good for Key West, Brest, and Boston applies also to New York and Penobscot bay, and is apparently due to an annual change in the ocean currents, but is not wholly explained even by this.

(13.) 1874.—The successive improvements in Ferrel's formulæ, and especially the fine series of observations in Penobscot bay, enabled him to show " why it is that satisfactory " and consistent values of the mass of the moon have not in " general been obtained from the semi-diurnal tides, for these " relations are disturbed by the various shallow-water com- " ponents which do not enter into the theory of deep-water

" tides as it has been used thus far in determining the moon's " mass. The perfection of the tidal theory so as to represent " accurately the results of observations at all tidal stations " and give a correct mass of the moon depends now mainly " upon the study of the shallow-water terms." This determination of the mass of the moon forms an interesting episode in Ferrel's elaboration of the tidal theory. His first computation of this mass was published in 1871, based on the tides of Boston, and is reproduced in chapter VII of his " Tidal Researches " (Cambridge, 1874), with extensive amplifications relative to the relations between deep-water and shallow-water tides.

This last-mentioned most important work on the tides, while summing up Ferrel's work up to the date of publication, does not represent all of the minutiæ subsequently elaborated by him. Besides containing the tidal formulæ in a form convenient for the use of tidal computers and the numerical relations that hold good strictly between the coefficients that enter into these formulæ, it contains numerous results of special discussions made by Ferrel and outlines the work that he still thought necessary and much of which he subsequently accomplished. In the concluding chapter of miscellaneous phenomena he gives the results of his discussion of the tides of Tahiti and, again, of the tides in Lake Michigan; these latter, as he was able to show, have an actual existence entirely in accord with his own tidal theory. From a geodetic point of view his discussion of the variation of the mean sea-level at various points along the shore of an ocean will always be of value, since he shows that in view of the effect of winds and currents we have a right to expect permanent differences of the mean ocean-level so great as to be easily perceived by accurate levelling operations; in other words, the mean sea-levels determined from tidal observations in a moving ocean do not coincide with the sea-levels of a stagnant ocean as represented by the liquid in the tube of a delicate level. Here, also, Ferrel rewrites his earlier results on the tidal re-

tardation of the earth's rotation and its application to the secular acceleration of the moon's motion. He shows that the earth must have an effect on the moon 6,000 times that of the moon upon the earth, and must therefore have long since forced the lunar rotation to coincide with its revolution about the earth.

(14.) 1874.—The closing paragraph of the "Tidal Researches" connects his paper of 1854 on the "Internal Fluidity of the Earth" with the present study of tides, and suggests that we have now succeeded for the first time in demonstrating a real cause as to why the exterior and interior of the earth should have different periods of rotation, and have thus taken one step toward rendering possible the truth of Halley's hypothesis of concentric magnetic shells and an internal nucleus to account for secular changes in the earth's magnetism.

(15.) 1871–1883.—Ferrel closed his work on tides for the Coast Survey by the invention and successful construction of a machine for predicting the maxima and minima of the tides. This machine is said to do the work of 30 or 40 computers and has been regularly used since 1884.

(16.) In meteorology Ferrel accomplished so great a work that it is impossible here to properly condense it, but I may enumerate a few more prominent items. His law connecting the velocity and direction of the wind with the barometric gradient was presented first to this Philosophical Society in 1874. It expresses the total effect of the two important considerations, namely, the rotation of the winds around a storm center plus the rotation around the earth's axis, and enables us at any time to compute the separate effects of the local and the general circulation of the atmosphere.

(17.) 1877–1881.—The general principles that control the development and progressive motions of storms were explained by Ferrel in his "Meteorological Researches," part I, and the detailed thermo-dynamic phenomena of storms were given in full in his "Meteorological Researches," part II.

The views elaborated in these two parts, as we all know, have marked an epoch in the history of meteorology, and have stimulated numerous other students of this science. Here, as elsewhere, Ferrel's work is marked by a rigid logic and a remarkable discrimination in selecting for consideration the important factors in the production of natural phenomena. These memoirs were further elaborated in 1885, in his "Recent Advances," and were presented in an unmathematical and so-called popular form in 1889 in his "Popular Treatise on the Winds." Ferrel held to the view that the progressive motion of a storm of any extent is controlled by the general circulation of the atmosphere, or, perhaps, I may with equal propriety say the storm is a part of and temporarily represents the general circulation as modified by local conditions. At the same time he was directly and consistently opposed to those who would attribute the fall of the barometer to the mechanical effect of the general circulation alone, since he gave us the correct formula by which to calculate the combined effect of general and local circulation.

Ferrel's elaborate article on waterspouts, tornadoes, and hail-storms has been widely quoted as particularly happy and convincing.

(18.) 1882.—In his memoirs on the "Conditions determining Temperature" in the atmosphere and at the earth's service, Ferrel achieved several advances, namely: First, the observed distribution of temperature in the northern and southern hemispheres was charted and formulated as preparatory to the theoretical computation of the pressure and resulting general circulation of the winds; second, the theoretical relation between the temperature shown by a thermometer and that of surrounding objects was formulated, whence he deduced the proper and best method of determining the true temperature of the air; third, the relation between the temperatures of the black bulb and bright bulb thermometers was investigated, whence he deduced the proper method of determining the amount of heat received from the sun

or other radiating body and gave tables for use with the Arago-Davy actinometer.

(19.) Applying the results of his previous studies on temperature to the determination of the temperature of the moon, Ferrel was able to give us a close approximation to the condition of the moon's surface and, again, a close approximation to the average temperature of our whole atmosphere at any elevation.

(20.) 1885.—Ferrel's studies in thermometry were made the basis of a special study on the whirled or sling psychrometer, whence were deduced hygrometric tables that were adopted by the Weather Bureau, and were undoubtedly a great improvement on those of Regnault, which had been in use up to that time.

(21.) 1886.—His "Meteorological Researches," part III, on "Barometric Hypsometry" were made the basis of a report on the best method of reduction to sea-level practicable for stations that must telegraph the reduced observations promptly to a central bureau. These tables of July, 1886, must be recognized as responding, so far as any way practicable, to the demands of our knowledge of the subject, and although criticised by some, still seem to me to admirably respond to the conditions of the problem—*i. e.*, the reduction of mountain stations, high plateau stations, and low stations all to sea-level by one uniform method; but this is not to acknowledge that such a reduction to sea-level is a step proper to be taken as compared with the reduction upward to a uniform high altitude.

(22.) 1889.—One of Ferrel's last contributions to science consisted in the discussion of all the observations and theories of the radiation of heat from solids—a subject that must be recognized as of prime importance in meteorology. The fragmentary experimental knowledge that we have on this subject was thoroughly sifted by him and the path for future experimenters marked out.

The sum total of the publications of our college aggregate about 3,000 octavo pages of formulæ and tables, representing

fully forty years of labor. We all remember his quiet ways, his indefatigable industry, his shyness, his perpetual absorption in the contemplation of some new and complex problem. He lived in an atmosphere of abstraction; he was with us, yet not of us. In 1886, at the age of 70, he resigned public office and prepared to retire to the rest of domestic life with near relatives. Among these he lived lovingly and died peacefully. His body lies in the cemetery at Waywood, Kansas.—It was mortal, but the example that he has left us—the ideas that he infused into science—will never cease to live. CLEVELAND ABBE.

EDWARD T FRISTOE.

[Read before the Society, May 26, 1894.]

PROFESSOR FRISTOE, son of Joseph and Martha Fristoe, was born in Rappahannock county, Virginia, December 16, 1827. He received his early training at the old-time country schools in the neighborhood of his home.

At the age of seventeen he entered the Virginia Military Institute at Lexington, Virginia, from which he graduated in 1849 with the highest honors.

For two years thereafter he was principal of an academy at Surrey court house, Virginia.

In 1852 he entered the University of Virginia and graduated in three years, receiving the degree of A. M. in 1855. He especially excelled in mathematics and natural sciences.

In 1855, while still a student at the University of Virginia, he was elected to the chair of mathematics in the Columbian College of Washington, D. C., which position he filled with great credit until 1860, when he resigned to accept the chair of mathematics and astronomy in the State University of Missouri.

He entered the Confederate army in 1862 as adjutant general of the Army of Southern Missouri, was made major in 1863, and soon after appointed colonel of cavalry. In 1864

he was with General Price in his march from the Arkansas river to the Missouri river.

After the close of the war in 1865 he was elected to the chair of chemistry in the Columbian College, District of Columbia, and in 1871 to the chair of chemistry in the Medical Department of the Columbian University; in 1872, lecturer on chemistry in the National College of Pharmacy, District of Columbia. In 1872, also, he received the degree of LL.D. from the William Jewell College of Missouri; in 1874, the degree of Phar. D. from the National College of Pharmacy, District of Columbia.

In 1884 the Corcoran Scientific School of the Columbian University, District of Columbia, was founded and Professor Fristoe elected professor of general and analytical chemistry and dean of its faculty.

At the time of his death he was professor of chemistry in the Medical Department of the Columbian University, District of Columbia, and professor of chemistry and physics in the Corcoran Scientific School and dean of the latter, and professor of physics and natural history in the Collegiate Department of the Columbian University.

In his character as a teacher he was faithful to all his engagements, and was personally a favorite with both students and with his colleagues.

He was an untiring worker, as his positions, added to practical work in the laboratory, indicate.

That he contributed so little to the literature of his special work is explained by the fact that all his time was occupied in filling his many duties as a teacher.

Professor Fristoe died of sunstroke, July 30, 1892.

It would be out of place to make this notice eulogistic, but I cannot close this sketch without a brief reference to his private character. He was a *gentleman* in the best sense of the word—an *upright, honest man*, a good citizen, a *true friend* to those who enjoyed the privilege of his friendship, and a *trusted, trustworthy teacher*.

He needs no eulogy.

D. W. PRENTISS.

JULIUS ERASMUS HILGARD.

[Read before the Society, October 15, 1892.]

When Joseph Henry presided over the first meeting of this Society, there were gathered around him, among its founders, many men of uncommon intellectual strength, not least among whom sat JULIUS ERASMUS HILGARD, the subject of this sketch.

He was a man of medium height, in maturity of substantial form. His head was round, his features small, and his kind gray eyes were well set under an ample forehead. His speech betrayed his foreign birth, for, notwithstanding that he came to this country as a mere lad of ten years, having been born at Zweibrücken, in the Palatinate, in 1825, he spoke with a marked accent. This was doubtless due to the fact that his youth was spent on a farm in Illinois, where intellectual intercourse was largely restricted to members of his own family, who, like himself, acquired the English language chiefly from books.

His father, who had studied jurisprudence in Paris and who had become imbued with the republican spirit of the times, gave up a promising and already successful judicial career in Bavaria under the mistaken impression that ideal social and political conditions were to be found on a remote farm in Illinois, whither he accordingly transplanted his large family. Being a man of cultivated mind and unusual talents, he successfully undertook the education of his children, instructing them in the humanities, but in the direction of the exact sciences he had to yield the teacher's place to his son and pupil Julius, owing to the phenomenal aptitude for mathematics displayed by the latter.

At the age of eighteen Hilgard went to Philadelphia, where, at the house of Judge Kane, he met Alexander Dallas Bache, who had then but recently been placed at the head of the Coast Survey. To this acquaintance his special interest in geodesy and connection with the Coast Survey

were due, for soon after in a letter he called Bache's attention to errors in the formulas in use in the Survey for the computation of geographical positions, giving his own development of correct ones. In reply Superintendent Bache wrote to the youth of nineteen, saying, "You have overridden two of our most experienced computers and have shown that they were seriously in error." To an offer from Bache of a position on the Survey in a subordinate capacity and at small pay he made the characteristic reply that he would rather do "high work at low pay than low work at high pay" and gladly accepted the position. His ability procured him immediate recognition, and, notwithstanding his youth, he was placed at the head of a surveying party in 1846 and made trigonometric, astronomical, and magnetic observations. His efficiency was as marked in the office as in the field, and the various duties assigned him were always discharged with marked ability.

For a brief period in 1861 he severed his connection with the Survey, but was recalled by Bache at the outbreak of the war, when the existence of the Survey was threatened by hostile legislation, to return to Washington and help save and maintain it. To his share of this task Hilgard at once addressed himself with his accustomed vigor and ability and succeeded by his cogent and farseeing arguments in convincing leaders of the dominant party of the necessity of maintaining such an organization in time of war. Called to assume charge of the Coast Survey office in 1862, the opportunity was afforded him to make good his prediction of its usefulness to the Government in those trying times by meeting the urgent demands made upon it for military maps, to the production of which the energies of the office were for a time mainly devoted. Then, when a lingering illness, not unlike that which finally clouded his own mind, incapacitated his chief, the administration of the whole Survey fell upon the shoulders of Hilgard in 1864. Then for two years he was superintendent, in fact though not in name, and filled the office, to quote the words of Benjamin Pierce, " with distinguished ability manifest to all."

After the appointment of a successor to Bache, Hilgard continued as assistant in charge of the office, giving much of his time to the office of weights and measures, which was practically under his direction. He took an active part in shaping legislation relating to the introduction into this country of the metric system, and prepared the standards of that denomination which were distributed to the various States of the Union.

In 1872 he made telegraphic longitude determinations between Europe and America, including in his operations Paris and Greenwich, and thus obtained the first reliable telegraphic difference of longitude between those great observatories, an operation to which it was his wont to refer as a diplomatic triumph. He also represented the United States officially as scientific delegate to the international convention convoked at Paris for the purpose of forming an international bureau of weights and measures. As vice-president he took an active part in the councils of the commission, and after the plans had been formulated and agreed to he was offered the directorship of the bureau, an offer marking the esteem in which his abilities were held by that body of eminent men composing the commission; but he declined it.

At the Centennial Exposition, held in Philadelphia in 1876, he acted as one of the judges on scientific and mechanical apparatus, being associated in that duty with some of the ablest scientists of Europe and America, and at about the same time he delivered a course of lectures at Johns Hopkins University on extended territorial surveying. He remained in charge of the office until the end of 1880, when he was appointed superintendent.

Hilgard's mind, in so far as his professional duties were concerned, was eminently practical. While directing large interests on the broadest plans, he grasped and gave attention to the minute and varied details of the work entrusted to him, introducing economies, by perfecting methods, in many ways, as, for instance, in substituting tapeline meas-

urement along the sandy beaches for the slow and expensive method of triangulation in vogue along the heavily timbered southern coasts.

While his strength lasted he was an indefatigable worker and student. In summer, early dawn found him at his books; in winter the rising sun put an end to his early morning studies, and during the day his active mind found no rest.

While carrying the evervarying burden of his official duties he found time to extend his sympathy, help, and wise counsel, for the two latter were much sought, to many persons in many ways, without as well as within his official sphere.

He was a member of many scientific societies; at one time president of the American Association for the Advancement of Science; a charter member of the National Academy of Sciences, and one of the founders of this Society, the earlier proceedings of which contain evidence of his versatility and scientific activity.

His life was in many respects burdened by misfortune, for of his four children three died young and only one lived to man's estate and then died, leaving him childless and overwhelmed by grief at a time when a fatal disease had already begun its inroads on his mental and physical strength. This disease had seriously impaired his health when he was appointed to the superintendency, which, to use his own words, came "too late." He was conscious of his failing strength and ability while still occupying the position of superintendent, for on more than one occasion he gave expression to the wish that the burden of his duties might be shifted to other shoulders.

His retirement took place in 1885, and from that time on his lingering illness entailed great sufferings, and several times brought him to the point of death. From each of these attacks he rallied with less power of resistance until death relieved him of his sufferings, on May 8, 1891.

O. H. Tittmann.

GARRICK MALLERY.

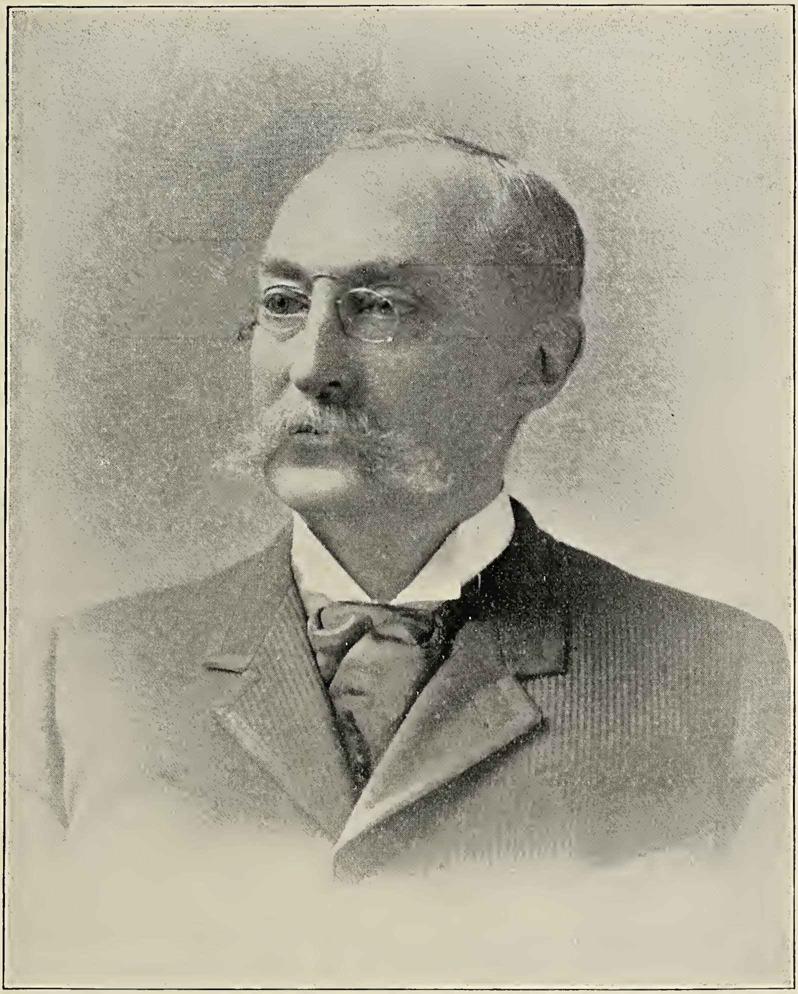
President of the Society in 1888.

[Read before the Society, February 16, 1895.]

Garrick Mallery was born in Wilkesbarre, Pennsylvania, April 25, 1831. His family was of English origin, he himself being in direct descent from Peter Mallery, who landed at Boston in 1638. Some of his ancestors were military officers in the colonial service, and at a later period others of them served in the Revolutionary War. With the easy indifference of those days as to the matter of spelling, the name was sometimes written Mallery and at other times Mallory. The name of Garrick had no association with the famous actor of that name, but was a very old family prænomen, having been at one time spelled Gayreck.

The father of our late fellow-member was Judge Garrick Mallery, who was born April 17, 1784, and graduated at Yale College in 1808. He was a member of the legislature of Pennsylvania from 1827 to 1830, and was distinguished for the zeal with which he promoted the reform of the prison discipline of the State. In 1831 he was appointed judge of the third judicial district, composed of the counties of Berks, Northampton, and Lehigh. He resigned from the bench in 1836 and removed to Philadelphia, where he practiced in his profession as a lawyer for many years. In the latter part of his career Judge Mallery held the office of master in chancery of the supreme court of Pennsylvania. He died in Philadelphia on the 6th of July, 1866.

Judge Mallery was distinguished as a jurist and was a man of broad views and cultivated mind. His high character and many accomplishments had a marked influence on the early training of his son Garrick. The mother of the latter, the second wife of the judge, was descended from John Harris, the founder of Harrisburg, the capital of Pennsylvania, and from William Maclay, the first United States



Garrick Mallery

From Photograph made in Sept., 1890.

Senator from that State. The elevating influences of these historic antecedents were not without their effect upon the generous mind of the young man. The inheritance of family traditions and the "cumulative humanities of some generations," as Dr. Holmes aptly phrased it, may take rank in a lower degree with the lessons of history learned at school.

Young Mallery received an excellent early education and, when the time came, was prepared by a private tutor for his entrance into his father's *alma mater*, Yale College, where he matriculated in his fifteenth year. His collegiate career was creditable to his abilities and industry, and he obtained more than one prize in languages and mathematics. He graduated in 1850. In 1853 he received the degree of LL. B. from the University of Pennsylvania. In the same year he was admitted to the bar in Philadelphia and commenced the practice of his profession in that city. He devoted some of his leisure time to editorial and literary work, and was steadily advancing in professional standing when the War of the Rebellion broke out in 1861.

Mallery, with the enthusiastic patriotism which characterized him through life, at once entered the volunteer army of the United States, and on the 4th of June was appointed captain in the Seventy-first Pennsylvania infantry. In the battle of Peach Orchard, Virginia, which took place June 30, 1862, Captain Mallery was very severely wounded and, being left on the battle-field unable to move, he was captured and sent to Libby prison, in Richmond. Upon being exchanged some time later he was sent to his home in Philadelphia, and upon his recovery returned to the field, and in February, 1863, was commissioned as lieutenant-colonel of the Thirteenth Pennsylvania cavalry. Colonel Mallery did excellent service throughout the remainder of the war, not only with his command but in various positions of importance to which he was assigned. He had speedily mastered the intricacies of military law, and his legal training, excellent judgment, and resolute courage were recog-

nized and made serviceable by those in high command. While the military occupation of the State of Virginia yet continued, Colonel Mallery was appointed judge advocate of the first military district. He subsequently acted as secretary of state and adjutant general of Virginia, which latter appointment continued until February 15, 1870. Colonel Mallery was honorably mustered out of the volunteer service in November, 1866, and at the same time accepted a commission in the regular army as captain, with an assignment to the Forty-third infantry. He received the brevet commissions of major, lieutenant-colonel, and colonel (the last in the volunteer service) for "gallant and meritorious services during the war." He was on duty in the office of the Chief Signal Officer of the Army for nearly six years, but his old wounds rendering him unfit for field duty, he was retired at his own request in 1879.

In 1870 Colonel Mallery was married to Miss Helen W. Wyckoff, daughter of the Rev. A. Voorhis Wyckoff, whose ancestors were among the early Dutch settlers of New York. Through her mother this lady was descended from Colonel Richard Townley, who came to this country with Lord Howard, governor of Virginia from 1684 to 1688. Colonel Townley married Lady Elizabeth Carteret, widow of Sir Philip Carteret, first governor of the province of New Jersey. Mrs. Mallery survives her husband, but there were no children.

At an early period in Colonel Mallery's career he became much interested in the Indian tribes with which he came in contact. His early reading had somewhat led him to the study of ethnology, and it was fortunate for himself and for the rising science of anthropology that he eagerly devoted himself to the researches offered by his surroundings. The ingenious Indian system of communication by signs and gestures attracted his attention, and he began to make a careful notification of them. This naturally led to the parallel investigation of their pictographs on rocks, skins, and bark, and he collected a large quantity of transcriptions of

these interesting records. He foresaw that these customs would ultimately be lost and forgotten as the Indians were brought more and more under the control of the authorities and were deprived of their favorite pastimes of warlike enterprise and hunting. Events have already justified his wise foresight. Before Colonel Mallery began his researches it was, perhaps, generally supposed that the rude pictographs of the Indians, some of which were believed to be of pre-Columbian time, were unmeaning and half-childish devices. He was gradually convinced that gesture-speech and the cognate pictographs formed a complete system, involving mythology and history and having an important relation to spoken language.

The Bureau of Ethnology was organized in 1874, and some time after the director, Major J. W. Powell, invited Colonel Mallery to pursue his investigations in connection with that institution. The first result was the publication, in 1880, of a pamphlet of 72 pages, with 33 figures, entitled "Introduction to the Study of Sign-language among the North American Indians as Illustrating the Gesture-speech of Mankind." This was intended rather as a manual for students, and in the same year followed a quarto volume of 329 pages, "A Collection of Gesture Signs and Signals of the North American Indians, with some comparisons." The latter work was distributed to collaborators only. In 1881 Colonel Mallery's second important contribution was published in the first annual report of the Bureau of Ethnology, namely, "Sign-language among North American Indians Compared with that among other People and Deaf-mutes." This treatise comprised 290 pages and was illustrated with 13 plates, a map, and 285 figures. While intended by its author as a preliminary report only, it at once took high rank, both at home and abroad, as an authoritative exposition upon an almost entirely new subject of anthropology. Mallery's next publication appeared in the Fourth Annual Report of the Bureau of Ethnology, 1886 (1887), under the title, "Pictographs of the North American Indians; a preliminary paper." It consisted of 256 pages, illustrated with

83 plates and 209 figures. This important work, the result of the parallel line of research in which its author had been engaged, met with immediate recognition and praise. Finally, the Tenth Annual Report of the Bureau of Ethnology, published in 1893 (1894), was devoted to Mallery's completed work on one of his subjects of investigation. It was entitled, "Picture-writing of the American Indians," filling 807 quarto pages, with 54 plates and 1,290 figures. This volume, with its opulence of illustration, is a noble testimony to the tireless industry, ingenious research, and power of philosophical comparison of its writer.

At the time of his death Mallery was preparing a treatise on the sign-language of the American Indians, intended to be a companion work to the "Picture-writing" last published. This, unfortunately, was left unfinished; but it is understood that it will be completed and published by the care of the Bureau of Ethnology.

In addition to the foregoing important and valuable series of writings, Mallery was the author of many erudite critical essays. In the Anthropological Society of this city, of which he was one of the founders and afterward president, some of these were read. Of these shorter ethnological writings may be mentioned "Manners and Meals," 1888; "Customs of Courtesy," 1890; "Greetings and Gestures," 1891. An essay entitled "Israelite and Indian; a parallel in planes of culture," 1889, which was published in the Popular Science Monthly, attracted much attention from its bold and ingenious comparison of two such widely dissimilar races, and a rather sharp controversy was the consequence.

Colonel Mallery was at one time president of the Literary Society of Washington, and his graceful essays read at its meetings proved his wide acquaintance with the literature of his own and other countries. He was for many years a member of the general committee of this Society and in 1888 was its president. His address on retiring from office had for its subject "Philosophy and Specialties." In it he insisted upon the importance of acquiring a correct and even elegant style of writing in scientific papers. His own compositions

were marked by ease and grace, and by great care in selecting words and terms which should be accurate in expressing the meaning intended.

Colonel Mallery died, after a short illness, at his residence on N street in this city, on October 24, 1894. He will be long remembered in this Society for his warm interest in its welfare and for his kindly disposition and genial manners.

ROBERT FLETCHER.

MONTGOMERY CUNNINGHAM MEIGS.

[Read before the Society, March 26, 1892.]

A man's biography is properly to be written by one who has intimately known him, if not through life, then at least at life's culminating point and beyond. I have known General Meigs, not intimately at any time, but only through a chiefly official intercourse in his old age, and I cannot ask you to listen to what you might expect from a comrade of the soldier's active years, or to much more than a brief summary of the dates and bare outline of a life which we have known at its evening, but whose noon was passed in the time of great events.

MONTGOMERY CUNNINGHAM MEIGS was a son of the eminent physician and medical author, Charles D. Meigs. He was born May 3, 1816, in Augusta, Georgia, and, entering as a youth the United States Military Academy at West Point, he was graduated from that institution in 1836, at the age of twenty. He was at once appointed to a position in the artillery service, and in the following year was transferred to the Corps of Engineers. In 1838 he was made a first lieutenant in that corps, and was detailed to direct the construction of Fort Delaware and afterward of other improvements on the Atlantic coast. In 1849 he was engaged in the Engineer Bureau at Washington, and in the latter part of 1852 was directed to make a survey at Washington, D. C., with the view of determining the best plan for supplying the city with water. He recommended the largest

and most abundant method of supply by damming the Potomac river at the Great Falls, some sixteen miles from the capital. This plan, approved by the War Department, was adopted by Congress, with the grant of liberal appropriations, and the projector of the enterprise became captain in the Engineer Corps, and to him was assigned the duty of designing and constructing the Potomac aqueduct. The watercourse of this great work was made to pass over several bridges, one of which surmounts a chasm 100 feet deep. Its single arch of 220 feet span still remains, I believe, the largest stone arch hitherto constructed.

During the prosecution of this important work (which occupied about ten years) Captain Meigs was also placed in charge, as supervising engineer, of the north and south extensions of the national Capitol and of the construction of its crowning iron dome, as well as of the northward extension of the General Post Office building.

Here he grew to a certain acquaintance with architecture which he could not have acquired only as an engineer. It is scarcely given to men to attain perfection in each of many different things and of opposite natures, and the science of the engineer and the æsthetic side of architecture are such opposites, but General Meigs possessed a sound professional knowledge of the principles of construction, and in this had a most important part of an architect's knowledge. He joined to it the agreeable talents of a more than ordinary skill in drawing and sketching in color, and his architectural work always and to the end of his life had to him an especial interest.

If there had been no civil war we might never have heard of him, except as a meritorious officer of engineers, who, in the routine of his charge, superintended various public works, and who, possessing the accomplishments of an unusually skilled amateur artist, designed, in conjunction with various architects, some of the prominent buildings of the capital.

But there was a civil war—a something which is already only history, perhaps, to the younger men who hear me—

but which to the older ones and to every American who has passed middle age, is filled with personal memories of things suffered and done, and of emotions which can hardly by any event be awakened in such intensity again.

Who that remembers the feeling excited by President Lincoln's first call for troops—the first realization by us of the North, that the inevitable had come, and that civil war was on us—can describe that mental breaking up of the fountains of the great deep? No one can describe it; but only its consequences; and these, with a new order of things, brought forward into universal recognition new merits in new men. Among these was Meigs, who had reached middle age in comparative obscurity, but who was now destined to take a prominent part in the public life and events of those hours when history was being made.

We here have all of us seen him in his old age; some of us, like myself, in his old age only, and I have tried to find from those who knew him in the culminating point of his activities, what he was like then.

One of my informants, himself a partaker in those events, tells me that General Meigs was at that time a man of remarkably fine presence and dignified carriage. The hair that we have known white was then notably black, and borne by a man in the vigor of middle life, whose conversation was agreeable and not devoid of a gentle humor. A man, said my informant, who made a good impression from the first.

Another witness, a soldier and junior comrade of Meigs, tells me that he was captain of engineers under circumstances and at a time when the position might have led him to the command of an army if not of our armies; but that when he became instead a brigadier general, in charge of the Quartermaster Department, he did not seek the place, but the place sought him, not only for his high integrity and acknowledged capacity for business, but on account of the strength of his personal character.

I know from others that in this position he had to defend the Government purse, not merely against fraudulent

or manifestly imprudent demands, but against specious ones, urged with the full pressure that "influence" of every degree could bring. He never yielded. Amid the convulsions of the world around him, he was immovable and inflexible as was the just man of Horace, and to those who know what it costs to be so in such circumstances, this is perhaps the most honorable part of the record of an honorable public life.

He must have been respected by his subordinates as by all who knew him; but if it be said that this inflexibility, which was the right and necessary quality in his place and at that trying time, was not calculated of itself to engage affection, I do not mean that he was one who could not be loved, but that it is part of the hardships of such an administrative position as his, and part of the temptation to soften inflexible justice to a man at heart as kindly as General Meigs, that he often cannot at the time be understood by those with whom he comes into brief, hard contact, though these may be his nearest official associates.

General Meigs would hardly have become what he did become, without a love of the Union which at one time made him an enemy of her enemies, not only in the field, but in word and act; but I should be forgetful, in speaking of his attitude to others and the qualities of his affections, if I did not add that I have personally experienced his essential kindliness at a time when its manifestation was very grateful to me.

In 1861 this till then obscure captain of engineers was brought to the notice of no poor or common judge of men—I mean Abraham Lincoln. I have spoken of the personally favorable impression that General Meigs then generally made, and this he made on the President, who, after a brief interview, decided to trust him with great powers. Let me here quote a few words from Hay's Life of Lincoln. It refers to this first interview with the President, momentous in all its consequences:

"The President talked freely with Captain Meigs and, after some inquiries about Sumter, asked him whether he could go down there again and take general command of

those three great fortresses, Taylor, Jefferson, and Pickens, and keep them safe. Meigs answered that he was only a captain and could not command the majors who were there. Here Seward broke in with, 'I understand how that is; Captain Meigs must be promoted.' 'But there is no vacancy,' answered the modest captain.

"Mr. Seward, however, made light of all difficulties and told the President if he wanted this thing done to put it in Meigs' charge. 'When Pitt wanted to conquer Canada,' Seward said, 'he sent for a young man whom he had noticed in the society of London and told him to take Quebec; to ask for the necessary means and do it, and it was done.' Would the President do this now? Two days afterward Meigs was about starting for church when * * * he was called to prepare a plan for relieving Fort Pickens, which was submitted that day to the President."

He was promoted to the rank of colonel of infantry and during the same year was raised to that of brigadier general, already referred to, and was appointed Quartermaster General of the United States Army, in which capacity a large part of the enormous expenditure of the whole civil war passed through his hands. I cannot enter into details, which are, nevertheless, an important part of our military history, but General Meigs disbursed what now seems a hardly credible amount—I have understood from him, something approaching one thousand millions of dollars—I do not say without a suspicion of his integrity (for that goes without saying), but with such almost unequalled knowledge of the technical difficulties imposed in Government accounts, that of this prodigious sum I have heard it mentioned that not ten dollars was disallowed at the Treasury.

Throughout the continuance of the civil war he administered the difficult duties of this most responsible position not only with success of the kind I have just mentioned, but in other ways. I will only allude to one, and for this again quote as my authority Lincoln's private secretaries, who, in their "Life," say:

"In September, 1863, General Meigs brought two army corps, numbering some 20,000 men, from the Rapidan to Washington, thence by railway through Cincinnati to the

Tennessee, with their guns, their munitions of war, and all their impedimenta, ready for fighting, in the almost incredible time of 8 days."

He served through the war with like signal skill, fidelity, and efficiency, and in 1864 received the well-earned title of brevet major general in the army.

With the war, ends the most active part of his life and begins the gentle course of an always honored old age, devoted, among other occupations, to those in which we have known him as an associate here.

He (from 1865) was a member of the National Academy of Sciences. He was one of the founders of the Philosophical Society, in 1871, having been a member of the Scientific Club, which preceded its organization. He was appointed, by joint resolution of Congress, in 1885, a regent of the Smithsonian Institution, and from his entrance into the board became an active member of its executive committee, all of which positions he filled until his death, which occurred at his residence, in this city, on the 2d of January of the present year, 1892.

Of General Meigs as a man, alike in external or in moral aspects, one can only speak in terms of respect. We have known him, even till his very last days, erect in his carriage, with a soldierly bearing which did not acknowledge the lapse of years, and a manner both dignified and engaging. In character, we have known one who was not only conscientious and sagacious, but firm, when firmness tried every quality of a man, at a time when there were done great things, all which he saw and part of which he was, who remained to the last courteous in intercourse and most judicious in counsel, and who proved capable of every care laid upon him throughout the varied duties committed to him during his long and honored life.

What more can we say, at its close, of this eminent and good servant of his country, than that he was a man faithful in all things?

S. P. Langley.

CHARLES HENRY NICHOLS.

[Read before the Society, October 15, 1892.]

Charles Henry Nichols, M. D., superintendent of Bloomingdale Asylum for the Insane, died at his residence, within the grounds of that institution, in the city of New York, December 16, 1889. He was the son of Caleb and Eunice (Kelly) Nichols, of Vassalborough, Kennebec county, Maine, where he was born October 19, 1820. Both parents were of old New England stock and were prominent members of the Society of Friends.

Charles, as he was usually called in his youth, was educated at the public schools of his native town and at an academy, conducted by Friends, in Providence, Rhode Island. From his youth he possessed marked individuality of character, was robust, resolute, and persevering. At the age of seventeen he began to teach school and continued teaching part of each year until he attained his majority, earning in this way the means to secure a higher education and a professional training.

After considering as maturely as he could the important question of selecting a vocation or profession, he found himself attached to medicine and made that his choice. The choice once made, he turned his attention earnestly to this study. He attended a course of medical lectures at the University of New York and a second course at the University of Pennsylvania, where he graduated as M. D. in 1843. On leaving college he began the practice of his profession in Lynn, Massachusetts. Here he met with encouraging success, but having an early leaning toward a study of diseases of the brain, he in 1847 became connected as an assistant physician to the eminent alienist, Dr. Amariah Brigham, in the New York State Lunatic Asylum at Utica. His associations with the staff of this hospital and studies pursued under Dr. Brigham made him a devoted student of mental disorders. He at the same time became greatly interested in the archi-

tecture and contrivances essential to secure the best hygienic results in hospitals with the greatest comfort to their inmates.

His sympathy for this unfortunate class thoroughly aroused his inquiring mind to examine into the history of legislation for the care of the insane, as well as their treatment in the past by the profession, together with a thorough and conscientious inquiry into their present condition and real needs in the United States, so as to deduce therefrom the most enlightened methods for their control and treatment. The library at the Utica asylum and others within his reach enabled him to pursue an exhaustive research into the literature of the subject and to view the facts from a medical, scientific, legal, historical, humanitarian, and philanthropic standpoint.

His earnestness and also his professional ability soon attracted the attention of co-workers in the field of this specialty, as well as of trustees charged with the management of institutions for the insane. As a consequence and probably growing out of a recognition of the soundness of his views on propositions for the improved management of hospitals equipped with better hygienic accommodations than then existed, he was, in 1849, invited to accept and was appointed physician and the superior officer to the Bloomingdale asylum, in the city of New York.

This was a prompt and complimentary recognition of the practical value of his studies and suggestions in his chosen field of work, while the conspicuousness of the position and the prominence of this hospital among those in the country placed a heavy responsibility upon the incumbent, from which he could not shrink. The vigor of a sound constitution, the habits of close application, and sympathy with his work enabled him to discharge his duty with credit to the hospital and to the satisfaction of the board of governors.

His zeal and energies were supported by a large and powerful frame. A well-disciplined mind regulated his hours and systematized his duties, enabling him then and

through life to perform a prodigious amount of labor. In addition to these duties of close personal attention he possessed in a marked degree the qualities essential to a good executive officer. He acted from reason, had a sound judgment, self-control, and a good memory. He was clear and explicit in giving directions and exacted of his agents specific reports on the business intrusted to them. He was himself attentive to details, full of resources in emergencies, met difficulties promptly as they arose, inspired to their best efforts all engaged with him. His purposes were ever benevolent and his aims lofty. To all his duties and engagements he was attentive, earnest, and persevering; a man of reason and reflection, of stable opinions and persistent, unceasing efforts to accomplish desired ends. He had a commanding manner, a gentle, kindly voice, persuasive in discourse, and many of the higher characteristics of a leader and reformer.

Doctor Nichols so administered the affairs of Bloomingdale asylum as to increase his already high professional reputation, and at the same time demonstrated his eminent fitness for the responsible duty of an enlightened superintendent and left no doubt in the minds of the governors that he was entitled to rank as one of the foremost alienists in the specialty. The Doctor never posed for admiration, but was a student and inquirer all the days of his life.

Doctor Nichols, having in his own estimation reached a point in the progress of his studies where travel for the purpose of examining and comparing the merits of home and foreign institutions and the various methods of caring for and treating the insane would be serviceable to him, and his private affairs demanding some attention also, resigned his position at Bloomingdale for these reasons in June, 1852.

He was not long left to indulge the dream of his personal preferences, but was almost rudely awakened to the consciousness that he was no longer an obscure physician, but had already, by his merits and studies, placed himself in touch with the foremost workers in the field of improvement

and reform in the care of the insane. He soon learned that for such true disciples there was no rest, and that no revelation of truth, no account of noble virtue is ever lost. His suggestions in the direction of amelioration had impressed themselves favorably upon the leading alienists of the country, as well as on some of those responsible under the laws for the care of this class of unfortunates. Men of the character, acquirements, and capabilities of Doctor Nichols were in demand, and before he had time to sail for Europe, or even adjust his private affairs, his services were sought for in an unexpected quarter.

President Fillmore invited him to an interview at the White House, which ended in his proffering to him the position of physician and superintendent of the hospital authorized by act of Congress to be founded in the District of Columbia for the care and treatment of the insane of the army and navy and of the marine hospital service of the United States and of the District of Columbia. This was a position requiring a man of comprehensive views and varied accomplishments in medicine, the arts and sciences, in architecture, and the hygienic requirements of a large and important Government institution that should meet in a satisfactory manner its present needs and further demands which were sure to be made upon it. Doctor Nichols had, unknown to himself, been recommended to President Fillmore by Miss Dorothea L. Dix and other prominent and influential persons throughout this country as eminently fitted by his character and studies for this responsible position. Reluctantly giving up his contemplated trip to Europe, he accepted the trust and came to Washington in October, 1852. He became a bonded officer of the Government and was placed in full charge by the Secretary of the Department of the Interior to select the site and to erect and equip the establishment authorized as a Government Hospital for the Insane.

Dr. Nichols was assisted in the selection of the site for the hospital by Miss Dix, and they certainly chose the finest

location to be had in the District of Columbia, being, indeed, unsurpassed by that of any institution in the country. The farm purchased contained 195 acres, was three miles southeast of the Capitol, and in full view of the city. On this land, with a frontage of half a mile on the Eastern branch of the Potomac river and having a good river landing, Doctor Nichols may be said to have erected everything needed for the institution. Here the brick were molded and burnt, the clay being taken from the excavations for the basements of the several wings of the institution. The hospital is constructed on the echelon plan, adopted by the Doctor, and was erected under his own immediate personal supervision. It was completed and furnished within the appropriation, and patients were received into it in 1853.

The gradually but steadily increasing number of patients to be cared for required within Doctor Nichols' time three several enlargements of the buildings, and the grounds were added to by purchases until they reached 420 acres, to meet the requirements of the stock, give pasture and range to the milch cattle, and furnish fresh garden vegetables to the institution.

This hospital, from its pleasing architecture, its ample wards, rooms, and corridors, and especially its superior ventilation, heating, and hygienic plumbing and sewerage service, has ever since its erection, with slight repairs, stood at the very head of the best hospital structures in all essential particulars and has been extensively modeled after by the several States and by some foreign countries.

"St. Elizabeth," the home name of the Government Hospital for the Insane (a name given in the original land patent and survey from the State of Maryland) stands today a grand and enduring monument to the sound judgment and large heart of Dr. Charles Henry Nichols. He has left the impress of his genius everywhere upon the property of the hospital, its grounds, its roads, its walks, as well as upon and throughout the buildings. The broad avenue which leads up to the institute bears his name, and at the back of

the stage in the hospital chapel is preserved a full length portrait of the Doctor, painted from life, as he appeared in good health and in the vigor of a superb manhood in 1877. This cherished memento was secured to the institution by his admirers. In the same chapel, to the left of the stage, hangs an oil portrait of Miss D. L. Dix, one of the greatest benefactors of the insane which our country has produced. This lady was a lifelong friend of St. Elizabeth.

From the time Doctor Nichols retired from the management of Bloomingdale asylum, and particularly after the Government Hospital for the Insane in the District of Columbia became an assured and admired success, the governors of the former institution never ceased to wish for his return to Bloomingdale.

In July, 1877, after repeated overtures from the governors of Bloomingdale and when they were contemplating extensive new buildings and improvements, about which he had been consulted, he was induced to consent to again accept service in that institution. His salary was fixed at five thousand dollars a year, with a spacious detached and fully furnished dwelling-house within the grounds suited for himself and family. The Doctor had a taste for the refinements and comforts of life, and he and his lady were always welcomed to the best society in Washington. Even with the increased salary and improved domestic comforts, it was with him a grave question to retire from an institution he had moulded into being and had watched over with such solicitude for a quarter of a century to its present grand proportions, where he had attended 4,000 patients and had everything in good working condition. It was but natural that he parted from St. Elizabeth as from a dear friend.

When he received the tract of land known as " St. Elizabeth " it was without buildings and an almost neglected waste. He left it a walled enclosure, with large and well-cultivated gardens and extensive, palatial hospital buildings, where were sheltered nearly a thousand patients, receiving the best of care and medical treatment known to Christian civilization and a generous government.

But while a great change has been wrought by his genius upon the beautiful heights overlooking the city of Washington, Doctor Nichols had himself grown almost unconsciously a quarter of a century older than when he selected the site for this noble institution.

Something of Doctor Nichols's capacity to transact business and his untiring energy may be inferred by a brief reference to the many duties he assumed or permitted himself to be drawn into outside of his regular hospital work, which was of itself sufficient to tax the energy of any ordinary man. During the war he was assistant surgeon to a hospital for volunteer soldiers, organized upon the grounds of "St. Elizabeth." He was at the same time and for years a member of the old levy court; one of the school commissioners for the county; a member of the board of police commissioners of the District of Columbia; a trustee and one of the executive committee of the Columbian University, District of Columbia; a member of the board of trustees of the Columbian hospital for women and lying-in asylum; a member of the Washington National Monument Society, and a life member and director in the American Colonization Society. He was well informed on almost every subject and fitted to take a prominent, if not a leading, part in any organization in which he was interested.

Doctor Nichols on resuming the duties as executive head in the affairs at Bloomingdale, in addition to his routine hospital duty, revised and greatly improved the methods and forms of keeping the histories and records of patients. He at the same time superintended the erection of a large wing, which quite doubled the capacity of the former building. He had for some years, at the request of the governors, been maturing plans for the erection of an entire new hospital, to be located on a farm owned by the society of the New York hospital at White Plains. The architecture, the material for construction, the size of wards, rooms, and corridors, and the arrangements for lighting, heating, ventilating, plumbing, as well as the sewerage of the institution, were to receive special consideration.

These details had been all worked out with much care, based upon study and the experience of a third of a century. His trip to Europe just before his last illness was undertaken in the interest and for the furtherance of this study. His life was spared him to complete the plans, report them to the governors, and to have the satisfaction of knowing that they were approved and adopted. The buildings are now in course of construction under the supervision of his esteemed former assistant, Dr. Lyon, now his successor as superintendent of Bloomingdale asylum.

The comparatively sudden break in the Doctor's habitual excellent health was, considering his age, something of a surprise to his friends; but a post mortem revealed the fact that a malignant disease of the stomach and other abdominal organs existed, which explains his great suffering and the fatal nature of his malady.

Doctor Nichols was proud of his profession and a supporter of its medical ethics. He was a member of the Medical Society of the District of Columbia since 1867; a member of the American Medical Association from 1858; the Association of Medical Superintendents of Hospitals for the Insane, of which he was president for a number of years; a member of the Centennial International Medical Congress, which met at Philadelphia in 1876, and also of the Ninth International Medical Congress, which met at Washington, D. C., in 1887. He was a member of the New York Academy of Medicine, the New York State Medical Society, the Medico-Psychological Association of Great Britain, the Philosophical Society of Washington, D. C., to which he was admitted May 4, 1872, and of other philanthropic and scientific associations at home and abroad.

Doctor Nichols received the honorary degree of Master of Arts from Union College, New York, and the honorary degree of Doctor of Laws from the Columbian University at Washington.

The Doctor was a good writer and an occasional contributor to the medical journals on subjects relating to his

specialty and on questions of hospital construction and management. He was the author of the Annual Reports of the Bloomingdale Asylum from 1849–1851, and also of the Reports of the United States Government Hospital for the Insane from 1852 to 1877.

Most of his papers have been printed in the American Journal of Insanity or in the Transactions of the Association of Medical Superintendents of Hospitals for the Insane; but much valuable work of his may be found in the clinical records and history, in the records of cases treated at Bloomingdale and the Government Hospital for the Insane, at Washington.

Doctor Nichols was twice married, first to Ellen G., daughter of the Hon. John W. Maury, mayor of Washington. She died in 1865, leaving a daughter, who did not long survive her, and one son, Maury Nichols, now a lieutenant in the United States Army, stationed at Willit's Point, New York, at the time of his father's death. His second wife, who survives the Doctor, is Sarah L., daughter of the late Prof. Alvan Lathrop, of Rochester, New York.

Doctor Nichols' remains were brought to Washington, D. C., and interred in a lot in the Congressional Cemetery, where the remains of his first wife and daughter lie buried.

J. M. Toner.

JAMES CLARKE WELLING.

President of the Society in 1884.

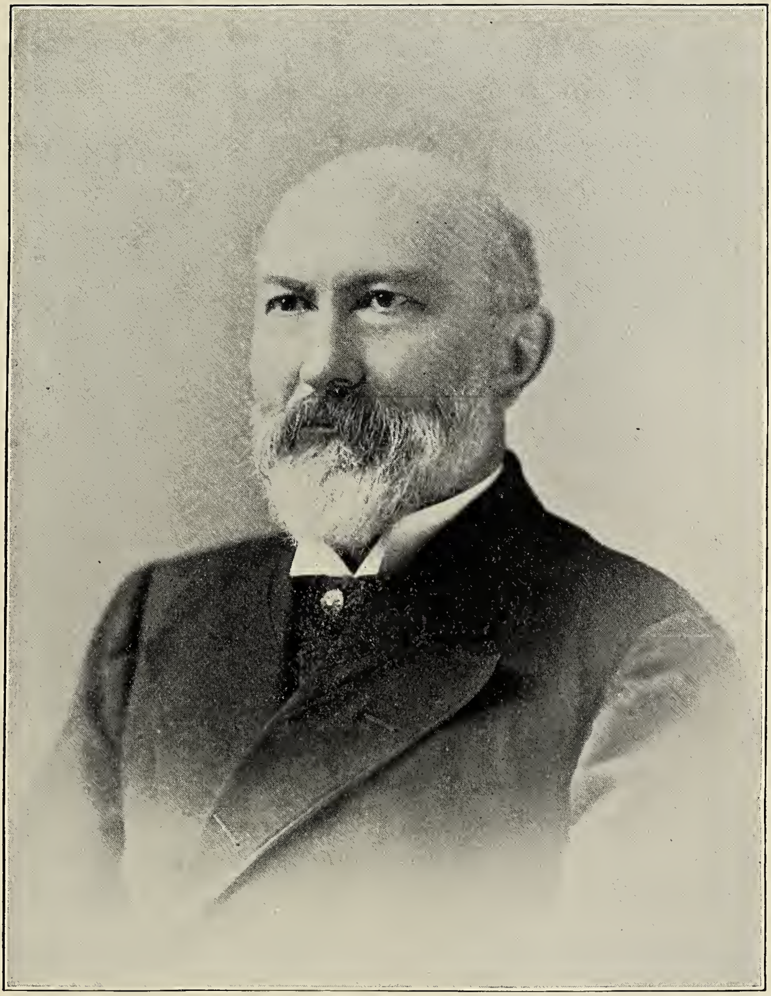
[Read before the Society, February 16, 1895.]

In the preparation of this short sketch it was assumed that the members of this Society would take more interest in the intellectual development of a departed colleague than in a mere survey of the incidental events of life ; hence some effort has been made to trace from words both written and spoken the growth of that personality revealed to this body—the personality of a scholar.

James Clarke Welling was born at Trenton, New Jersey, July 14, 1825. After completing an academic course he entered Princeton College, from which he graduated in 1844, the youngest member of a large class, many of whom subsequently achieved distinction in literature, politics, or the legal profession. Even while a student he had fixed upon the practice of law as his lifework, but realizing that the expense attending his preparation for that profession must be met by himself he accepted the position of private instructor in a Virginia family and entered upon the discharge of his new duties immediately after his graduation.

In starting out as the maker of his own fortune there were two incidents connected with his first visit to Virginia which made a lasting impression upon the student already imbued with the spirit of patriotism and a fondness for history. He found that his new home was " Wakefield," the birthplace of Washington, and his arrival there was on July 4. In speaking of this in subsequent years he referred to his feelings when he stood that night, a stranger amongst living strangers, beside the graves of those who had given to the world one who had become a stranger to none.

During the four years of residence here he pursued the study of law with zeal, but when the question of practicing his elected profession faced him, with its uncertainty of im-



James C. Welling

From Photograph made in 1892.

mediate success and the delays to which a young lawyer must inevitably submit, they, in view of his recent marriage, took on a serious aspect and prompted him to accept the position of associate principal in the Carlier Institute, in New York city.

His interest in school-work and the pleasure of authorship into which he drifted banished for a time, as he thought, the original purpose of his life; but the influence of these early studies in law was always with him, showing itself first in his editorials treating of international questions, then in his lectures on the laws of nations, and finally in the outline he drew of a School of Comparative Jurisprudence. At this institute it was his duty to give instruction in Latin and Greek. As a conscientious teacher he carefully prepared the lesson for the morrow, but, as a student storing up knowledge for the future, he committed to memory the text for each recitation. This will explain to those who have marvelled at the richness of his quotations and their aptness that he was giving a part of himself, not an acquisition for the specific occasion.

His contributions to the press just referred to, because of the correctness of style, beauty of diction, and vigorous reasoning, attracted attention and ultimately resulted in an invitation, in 1850, from Messrs. Gales and Seaton to assist in the editorial work of the "National Intelligencer." This paper was then a recognized power in the entire country, and although at one time Republican in politics and afterwards an exponent of Whig principles, its editorials, frequently exhaustive essays upon current topics, were recognized as reflecting the most reliable expressions of dominant principles.

In 1856 Mr. Welling became associate editor in name, though in reality a larger part of the conduct of the paper fell to him. It now became more pronounced in its endorsement of the Whig party, though upon many subjects it asserted its independence. In glancing over the files of the "Intelligencer" marked by him, it is exceedingly inter-

esting to note that many of his articles, in spirit contrary to popular opinion, were in so far prophetic that subsequent events placed upon them the seal of verification. Among these may be mentioned a discussion of the Monroe doctrine, the principles of international law involved in the seizure of Mason and Slidell, and the constitutionality of military commissions for the trial of citizens in loyal States. Although bound to the South by the closest ties, he resisted with all the strength at his command the movement toward disunion. On the other hand, he questioned the validity of the Emancipation Proclamation, but advocated Lincoln's proposal to free the slaves with compensation to loyal owners. Because of his connection with a Virginia family, he found as congenial friends many of the Southern sympathizers living in Washington, prominent among whom was Mr. Corcoran. As a far-seeing, conscientious writer, it was not easy task to discuss the burning questions then at issue, and more than once did the views he expressed awaken feelings of regret, if not resentment, in the hearts of some of his friends. Fortunately, in the large majority of cases the ultimate vindication of his opinions strengthened these tested friendships, and, although Mr. Corcoran confessed to having frequently wept over the editorials of Mr. Welling, they afterwards became the firmest friends, and the latter in many ways felt the loyal devotion of the former.

It is hardly safe to say that the "Intelligencer" dictated to the Government any definite policies, but it is known, to a few at least, that the President on more than one occasion saw fit to solicit an interview with Mr. Welling before he prepared his editorial comments on a declared line of action. Just what form these interviews took and who was the convincing person is not known, for of such marked distinction it was not Mr. Welling's wont to boast.

After nearly fifteen years' connection with the "Intelligencer" he withdrew from journalism and sought rest in foreign travel. Upon his return in 1866 he was appointed clerk of the United States Court of Claims, a position which

he relinquished in the following year to accept the presidency of St. John's College, at Annapolis, Maryland. His entry upon his duties there was at a critical time in the history of the college. An organized attempt had been made to withdraw certain grants that had been regarded as perpetual, and in the resulting uncertainties as to definite policies the attendance had fallen to ninety students. However, the new president brought fresh life and vigor to the institution and he had as his reward the pleasure of seeing the roll contain two hundred and fifty names. While here Columbian College conferred upon him the degree of LL. D.

Princeton College had not lost sight of her gifted alumnus, and when an opening was found in the curriculum for a course in *belles-lettres* he was, in 1870, called to conduct that department. Upon the occasion of his installation he delivered an address on "The True Sources of Literary Inspiration." It was full of the ripest wisdom and glowed with the best thoughts of the ablest men of all times. The theme running through this address was that the science of the rhetorician can only regulate and direct, chasten and subdue. It cannot create, for it finds the very conditions of its existence, as also the form and substance of its contents, in those antecedent creations for which it undertakes to account on logical and rational grounds. The science of rhetoric may analyze and explain the nature of human discourse, the sources of its power, and the number and quality of its different effects, but it cannot impart the energizing forces which shall set the mind of a Milton or a Burke in motion. Science may explain to the literary artist the function and names of the tools with which he is to work, but all the rules of all the rhetoricians cannot give him dexterity in the use of them. This must come, the favor of Minerva being presupposed, from long and patient practice in the actual exercise of literary or oratorical art. A literary taste which shall be at once delicate and correct can come only from a felicity of nature which has been trained by culture, fed by long meditation on the ideal forms of beauty in the soul of man,

and refined by communion with the manifestations of the beautiful in nature and art till in the end the æsthetic sense within us shall be as feelingly alive to each impulse as was the untaught soul of Correggio when, for the first time, he stood in the presence of Raphael's St. Cecilia and exclaimed, "Anch' io sono pittore!"

With all the skill of the logician this theme was developed, and the aptest quotations from ancient and modern authors called to prove that the poet "does not sing by the critic's score, albeit that he follows implicitly the subtle laws of rhythm and melody."

He entered with zeal into his work at Princeton, having faith in his *alma mater* and looking upon that noteworthy fact in the history of the race that all periods of great civil commotion have been attended and followed by a new phase of intellectual energy. "It is," he was accustomed to say, "in the stir of great events and under the sway of strong passions that the dormant powers of the national mind are roused into vigorous exercise, and though in the stern gladiatorship of martial conflict these powers may for a time be laid under contribution by the demands of a mere material strife, the day soon comes when an antagonist must needs retire from the lists, when arms must yield to the gown, and when the statesman and the scholar are called to untie with dexterous hand those Gordian knots which the sword left uncut."

He filled this position so congenial to his tastes and acquirements only one year, when he was called to the presidency of Columbian College. On the occasion of his induction into this new office he spoke upon "The Fundamental Elements of Intellectual Education," in which the dogma is laid down that "no university, even in all its faculties, can teach all knowledge, but it can comprise in its curriculum such a compend of the past thought and cultivation of the race as shall be reduced to the shape and dimensions best fitted to be taken in by the minds of the present generation, and therefore best fitted to promote the growth of culture.

Hence it is easy to see that the number and quality and arrangement of studies in a university course are not arbitrarily fixed by educators, but are the outgrowth of man's intellectual tendencies in the past and the highest expression of his intellectual wants in the present, as they are the indispensable conditions of future intellectual progress." In this address was foreshadowed the belief that in the near future advantage would be taken of the felicity of position and the Columbian college become a university."

He at once put in motion those forces which he thought best suited to bring about this ultimatum, securing first of all the adoption by the Legislative Assembly of the District of Columbia of an amended charter which permitted the college corporation to become a selfperpetuating body, then the passing by Congress of an act providing, amongst other changes, that the corporation " shall hereafter be known and called by the name of The Columbian University."

Acting on a suggestion received from Mr. Corcoran, he recommended that measures should be initiated for raising a permanent endowment fund. The steps then taken resulted, through many windings and hindrances, in the establishment of the Corcoran endowment, which constituted the first working endowment the University ever had.

It is well to note that in his annual and special reports to the University authorities and in his presentation of the claims of the institution for larger support or better endowments he gave, with the insight of a philosopher, a just estimate of the wealth of available material in the scientific plant of Washington, the impulse given to higher work by the advanced investigations here prosecuted, and the aid that could come from the army of scholars who make this city their home.

His presidential duties were very absorbing and left him but little time for extra-university work. However, we find him in 1877 accepting membership in the board of trustees of the Corcoran Gallery of Art. In 1884 he was made a regent of the Smithsonian Institution, and in the same year

delivered, as the retiring president of this Society, an address on "The Atomic Philosophy, Physical and Metaphysical." Starting from the atomic philosophy of the Greeks as a point of departure, and showing that this philosophy was not a mere exhalation of the imagination, but a logical inference from the major premise of their natural metaphysics, he traced with the clearness of a scientist and the accuracy of a historian the development of the atomic theory, touching, in passing, every contributor to this great subject down to Tyndall and Dalton.

He showed that when the law of simple multiples in chemical combinations was given to the world by Dalton, and was expressed by him in atomic language, he had really made a great departure from the physical methods of Democritus; "though," said he, "it is curious to observe that there is a perfect identity between the metaphysical ideas underlying his logic and the metaphysical ideas underlying his Greek predecessor. The method of each proceeds on the assumption of the indestructibility of matter, and it is from this platform that the English chemist reaches out his hand to the Greek philosopher in token of a common metaphysic."

He realized that it was an honor to be chosen to preside over a society which had had Henry, Newcomb, Taylor, and Powell for presidents, and that his retiring address should be no trivial effort. It only needs a glance at the foot-notes to show one that in its preparation his study had been profound and his reading varied.

The year 1884 saw the realization of one of his most cherished hopes—the opening of the Corcoran School of Science. In 1887, at the request of Mr. Corcoran, he visited the art galleries of Europe and brought back, in addition to some paintings, ideas to serve as the basis of the projected Corcoran School of Art.

One of the results of his early studies in law, with the subsequent delving into the principles of international relations preparatory to discussing them editorially in their

immediate application to some question of the day, was the appreciation of the importance of the establishment of a School of Comparative Jurisprudence, in which the law of the civilized world should be taught as a history and as a philosophy, "from the first rude germs of the clan stage of human government up to the highest evolutions of that international law which today sits supreme above all polities and all conventions of men."

In 1891 he published a comprehensive scheme for such a school and received for it the hearty endorsement of the ablest jurists of the world.

The first years of this decade showed great literary activity on the part of Doctor Welling, resulting in a paper, read before the Anthropological Society of Washington, on the "Law of Malthus." As usual, he began with history, showing that there were Malthusians in the world before Malthus; that Plato proposed to regulate the conditions of marriage in his ideal republic, and Aristotle suggested that the number as well as the quality of the offspring within the state should be subject to restrictions.

Another paper of this period was read before the New York Historical Society, upon "Connecticut Federalism, or Aristocratic Politics in a Social Democracy." The State of Connecticut was selected for this discussion "because of the greater simplicity of her social tissue during the early colonial period; because of her priority in ordinating under the new conditions of American life a purely independent and popular form of self-government; because of the preëminence she had, through her representative men in the Federal convention, in determining the structure of our federative policy; because of the active share she took in dressing the balances of the Constitution in the fateful matter of slavery, thus helping to contrive the sectional equilibrium which she was afterward destined to shake, and, finally, because of the greater tenacity with which she clung to the Federalist party in its origin, in the period of its proud ascendency, and in the days of its decline, down to the hour of the 'dim eclipse within the closed doors of the Hartford convention.'"

We see in this scholarly paper evidences of that rich equipment which would have made so valuable his "Civil Side of the Civil War," a history whose groundwork was laid, for whose completion rest from routine work was sought, and for whose interruption death alone was responsible.

Historical topics now occupied his attention, chiefly, perhaps, because at this time he was looking over his wealth of material in anticipation of putting it in final shape. His papers were: "The Law of Torture; a Study in the Evolution of Law," delivered as the presidential address before the Anthropological Society. It is here that we find the succinct statement that history, under the domain of the comparative method as projected along the lines of a continual evolution, is passing from a philosophy teaching by example into a science teaching by verified principles—from a philosophy teaching by analogies into a science teaching by homologies—showing that as a student of history he thought he perceived principles broad enough to serve as foundations upon which predictions might be safely based. This conviction found more definite expression in the address on "The Science of Universal History." In this category of papers may be included one read before the American Historical Association on "Slavery in the Territories;" also a second presidential address before the Anthropological Society, in which he gave an account of the last town election in Pompeii, "An Archæological Study of Roman Municipal Politics based upon Pompeiian Wall Inscriptions," interpreting many of the curious fragmentary writings and broadsides in the light of his accurate knowledge of Roman law, politics, and institutions.

The occasion of the Bering Sea arbitration called forth a paper on the subject of "Pelagic Sealing Juridically Considered According to Particular Analogy of Municipal Law." This principle, firmly established in early Roman law and given by Gaius, is the disposition on the part of animals to return.

The plea ultimately based upon the principle named is as follows: The Government of the United States avers that the Alaskan seal is an amphibious animal, which has its fixed home on Alaskan islands, and that from this home it never long departs, because of its fixed *animus revertendi;* that this fixed *animus revertendi* gives to the breed an economic aptitude of great value, and that however far they may roam from land they can still be definitely related to the soil on which they increase and multiply.

As has been stated, Dr. Welling, at the time of his acceptance of the presidency of Columbian College, placed directly before him the growth of the college into a university, not that he deemed the aggregation of the four faculties as constituting a university—he dreamed of something more, and cherished this dream as a waking principle and, working by its guidance for twenty years, succeeded in putting in place the completing element of the university, the School of Graduate Studies.

On the evening of October 4, 1893, many of his friends and colleagues who had shared with him the aspirations for this consummation now joined our joy of realization with his as he inaugurated this new school. On this occasion he spoke with unusual vigor upon "The Science of Universal History, its Methods and its Relations to the Physical Sciences."

It was a fitting theme for one whose studies along that line had been fraught with such success. It was most ably treated, and is, as if by purpose, a capstone and crown of an active scholarly life.

His walk among us came down to such a recent date that it would be useless to speak of the impress which his culture and genial nature have left upon us. Suffice it to say that wherever we find a pupil who lived close within the light of his thought, upon such an one we see the world's signet of approval as a broad and tolerant mind.

Coëxisting with the administration of a university in fact as well as name was the thirst for universal culture—

a thirst which in its gratification kindled yet another, and in this cyclic desiring and acquiring his life was passed—a life whose ending has brought to many of us a juster conception of his great learning and a feeling of personal loss as we become conscious that this great store of knowledge is beyond our enjoying.

Feeling that the administrative work of his position was making too great a draught upon his time and strength, he asked to be relieved of his presidential duties. His request was granted, and with the close of last session he saw beginning the opportunities he had longed for—time in which to put in shape for publication several works which lacked only final revision. To this task he had looked forward with joyous anticipations, and in sympathy with him, cognizant of the mines of information from which he could draw, many of us were impatient to herald the printed volumes.

On September 4, the very eve of his intended return to Washington to take up this labor, there came without warning the unexpected messenger of death. He received it with imperturbed serenity, as he received all of life's joys and sorrows. It found him as did every duty—ready.

J. H. Gore.

CHARLES CHRISTOPHER PARRY.

[Read before the Society October 15, 1892.]

Charles Christopher Parry was born at Admington, Worcestershire, England, August 28, 1823, and died at Davenport, Iowa, February 20, 1890. His parents were in moderate circumstances, and to better their condition came to America in 1832. They settled on a farm in Washington county, New York, in a locality since made classical by the labors of Dr. John Torrey. Parry early became acquainted with Drs. Torrey and Gray, then engaged in the preparation of their "Flora of North America," and, stimulated and encouraged by their friendship, he imbibed that love of nature which was ever afterward the actuating impulse of his life. He graduated at Union College, Schenectady, New York, and afterward took a medical course at the College of Physicians and Surgeons, in New York city.

At the age of twenty-three he moved to Iowa, settling at Davenport, which thereafter remained his home, so far as he had any abiding place. So far as known, he practiced his profession for a few months only. In 1848 he began his real work as an explorer by becoming associated with David Dale Owen's geological survey of the northwest. His first collections were made along the St. Peter river and up the St. Croix as far as Lake Superior. In 1849 he was appointed botanist to the Mexican boundary survey, and the results of his indefatigable enterprise are recorded in the publications of that survey. His first field work with this survey was a voyage by way of the isthmus of Panama to San Diego, California. This was repeated in 1850, owing to the loss, in a storm, of the previous collection. In 1851 he visited El Paso, on the Rio Grande, and made extensive collections in a region never before and rarely since visited by a botanist.

In 1861 he began, at his own expense, a series of explorations in the Rocky Mountain region, particularly the higher

mountain peaks of Colorado. This region was then almost unknown botanically, and Dr. Parry fairly reveled in the untrodden field and brought to scientific attention numerous of its beautiful alpine plants. Those botanists who are so fortunate as to have visited a high mountain peak and seen in its native state the delicate, bright-colored floral wealth can well appreciate the enthusiasm of a botanist who, like Dr. Parry, had the good fortune to visit many an alpine summit where almost every second plant was new to science. It was an experience that has fallen to the lot of few botanists, and it is little wonder that after this, exploration became his passion. Perhaps one of the pleasantest incidents of this work was the discovery of the beautiful primrose that bears his name (*Primula Parryi*).

In 1867 he became attached as botanist to that one of the Pacific Railroad surveys which crossed the continent on the thirty-fifth parallel. His abundant collections were described in conjunction with Dr. Torrey.

In 1869 Dr. Parry was appointed botanist of the Department of Agriculture, a position which he held for three years. His work while thus connected was mainly of a routine character, such as identifying and caring for the great collections which had begun to pour into the national repository. It was work of a scientific technical nature, so much so, in fact, that it was made the basis of action for his removal. That his discontinuance in official life was regarded as a great loss to botanical science is shown by the public letters of Dr. Asa Gray and others written at that time.

It was while in Washington that Dr. Parry was elected to membership in the Philosophical Society. He was elected May 13, 1871, but, so far as the records show, did not take part in any of its meetings. However, he remained in Washington only a short time after his election, else the Society would doubtless have been enriched, as was the Davenport Academy of Science, which he helped to found, by contributions from his abundant resource of exploration and discovery.

In the season of 1878 Dr. Parry made, in company with Dr. Edward Palmer, the first of his memorable botanical excursions into some of the less known parts of Mexico. The valuable collections there gathered were made the basis of a series of elaborate descriptive papers by Dr. Gray. So fascinated did Parry become with exploration that the remaining twenty years of his life were entirely devoted to it, and he visited and made collections in nearly all of the western States and Territories. These collections were so exhaustive and so carefully prepared that he came to be regarded as the most distinguished field student of his time. Though he wrote but little, our western flora cannot be touched at any point without encountering the name of Parry. Hundreds of species new to science were brought to light by him, and more than fifty species bear his name. There is hardly a characteristic western genus of plants but has a species named in his honor. That he possessed the faculty of keen, philosophical discrimination is shown by his treatment of the difficult genera *Ceanothus* and *Choryzanthe.* He was unusually well fortified by a wealth of acute field observations, and having studied in a living state most of the plants he monographed, was well qualified to pronounce on many vexed questions of relationship. It is probable that had he lived a few years longer he would have been prevailed upon to put on record his views on many obscure questions of plant affinities.

In stature Dr. Parry was below the medium, being about five feet six inches in height. In complexion he was fair, with hair and beard well silvered at the time of his death. As a companion he was kindly, genial, and self-sacrificing; as a botanical explorer he was indefatigable, acute, and thorough, and nothing escaped him, and as a student he was careful, discriminating, and eminently judicial.

F. H. KNOWLTON.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON

AND OF ITS

MATHEMATICAL SECTION

1892-1894

PROCEEDINGS

AT THE

GENERAL MEETINGS OF THE SOCIETY.

1892 TO 1894.

FROM THE MINUTES.

379th Meeting. **January 2, 1892.**

The President, Mr. G. K. GILBERT, in the chair.

Thirty members and guests present.

Announcement was made of the death, at his residence in this city, at 5 a. m., on January 2, 1892, of General MONTGOMERY CUNNINGHAM MEIGS, one of the founders of the Society.

The President announced the following standing committees of the Society for 1892.

On Communications:

R. S. WOODWARD, *Chairman*. J. S. DILLER. F. H. BIGELOW.

On Publications:

ROBERT FLETCHER, *Chairman*. W. C. WINLOCK. MARCUS BAKER.

The report of the committee appointed to audit the accounts of the Treasurer for 1891 was read and adopted.

Mr. F. H. NEWELL read a communication on Fluctuation of discharge of western rivers. [Published in Thirteenth Annual Report of the Director of the U. S. Geological Survey. 8°, Washington, 1893, part 3, pp. 1–99.]

He was followed by Mr. J. R. Eastman, who read a paper on The Mexican meteorites. [Published in this volume, pp. 39–52.]

This paper was discussed by Messrs. Hill, Gilbert, Merrill, Bigelow, Paul, Mendenhall, and Eastman.

380th Meeting. **January 16, 1892.**

President Gilbert in the chair.

Twenty-five members and guests present.

Announcement was made of the election to membership of George Rockwell Putnam, of the United States Coast and Geodetic Survey.

Mr. W J McGee read a paper on The Gulf of Mexico as a measure of isostasy. [Published in American Journal of Science. 8°, New Haven, 1892, Sept., 3d ser., vol. 44, no. 261, pp. 177–192.]

It was discussed by Messrs. Gilbert, Abbe, Harkness, Woodward, Taylor, Hill, Willis, Dall, Christie, and McGee.

Special Meeting. **January 20, 1892.**

President Gilbert in the chair.

Attendance, about two hundred.

By the courtesy of the authorities of the Columbian University a special meeting was held in the lecture room of that institution to listen to the annual address of the retiring president, Mr. T. C. Mendenhall. His subject was The uncertainty of conclusions. [Published in this volume, pp. 1–18.]

381st Meeting. **January 30, 1892.**

President Gilbert in the chair.

Seventy-five members and guests present.

By invitation, Dr. Joseph Le Conte, President of the American Association for the Advancement of Science, read a paper

on The relation of philosophy to psychology and to physiology. [Published in this volume, pp. 19–38.]

The paper was discussed at some length by Messrs. Ward, Harris, Powell, and Le Conte.

On motion of Mr. Abbe, a vote of thanks was tendered to Dr. Le Conte for presenting his paper to the Society.

382d Meeting. **February 13, 1892.**

President Gilbert in the chair.

Forty members and guests present.

Announcement was made of the death, at his residence in New York city, on February 9, 1892, of the Hon. John Jay Knox, a member of the Society.

Mr. Gardiner G. Hubbard read a paper on The history of the education of the deaf.

It was discussed by Messrs. Gallaudet and Nott.

Mr. A. W. Greely read a paper on Some peculiarities in the rainfall of Texas. [Published in this volume, pp. 53–66.]

Remarks were made by Messrs. Harrington, Hazen, Curtis, Hill, and Greely.

383d Meeting. **February 27, 1892.**

President Gilbert in the chair.

Fifty members and guests present.

Mr. M. W. Harrington read a paper entitled Notes on the climate of Death valley. [Published with the title U. S. Weather Bureau, Bulletin No. 1. 8°, Washington, 1892, 50 pp.]

It was discussed at some length by Messrs. Greely, Merriam, Thompson, Farquhar, Palmer, Finley, and Harrington.

Mr. L. A. Bauer read a paper on Wilde's explication of the secular variation phenomenon of terrestrial magnetism. [Pub-

lished in American Journal of Science. 8°, New Haven, 1892, June, 3d ser., vol. 43, no. 258, pp. 496–508.]

There being no time for discussion, the meeting at 10.05 adjourned.

384th Meeting. **March 12, 1892.**

Vice-President Fletcher in the chair.

Thirty-six members and guests present.

Mr. B: Pickman Mann read a paper entitled An attempted solution of a social problem. [Not published.]

It was discussed by Messrs. Bigelow, Dall, Bacon, Riley, and Mann.

Mr. A. S. Christie read a paper entitled Remarks on the diurnal variation of the barometer. [Published in this volume, pp. 67–70.]

It was discussed by Messrs. Greely, Harrington, Hazen, Bigelow, Mendenhall, and Christie.

385th Meeting. **March 26, 1892.**

President Gilbert in the chair.

Fifty-five members and guests present.

Mr. S. P. Langley read a biographical notice of General M. C. Meigs. [Published in this volume, pp. 471–476.]

Mr. L. F. Ward read a paper on The new psychology and what it promises. It consisted of a condensation of part 2, chapters 1–29, of a volume afterwards published under the title The psychic factors of civilization. 8°, Boston, Ginn & Co., 1893.

It was discussed by Messrs. Powell, Ward, Harris, and Riley.

386th Meeting (Special). **April 6, 1892.**

President GILBERT in the chair.

Twenty members and guests present.

Mr. THOMAS RUSSELL read a paper on River stage predictions. [Published in volume entitled Weather and methods of forecasting. 8°, New York, Macmillan & Co., 1895; also in the American Meteorological Journal. 8°, Boston, 1892, June, vol. 9, no. 2, pp. 47–58.]

It was discussed by Messrs. HARRINGTON, NEWELL, and GILBERT.

Mr. J. P. IDDINGS read a paper entitled A study of a dissected volcano. [Published in the Journal of Geology. 8°, Chicago, 1893, Sept.–Oct., vol. 1, no. 6, pp. 606–611.]

It was discussed by Messrs. VAN HISE and GILBERT.

Mr. WALDEMAR LINDGREN read a paper on the Silver deposits of Lake valley, New Mexico.

387th Meeting. **April 9, 1892.**

President GILBERT in the chair.

Thirty members and guests present.

Announcement was made of the election to and qualification for membership of FREDERICK HAYNES NEWELL.

Mr. W J McGEE read a paper entitled Illustrations of isostatic pressure. [Not published.]

Mr. BAILEY WILLIS read a paper entitled Illustrations of Appalachian structure. [Published in a paper entitled The mechanics of Appalachian structure, which appeared in the Thirteenth annual report of the Director of the U. S. Geological Survey. 8°, Washington, 1893, part 2, pp. 211–282.]

It was illustrated by lantern views, and was discussed by Messrs. WALCOTT, WOODWARD, and McGEE.

Mr. Robert T. Hill read a paper on The geologic evolution of the topography of Texas. This paper also was accompanied by lantern views.

388th Meeting. **April 23, 1892.**

President Gilbert in the chair.

Twenty-five members and guests present.

Announcement was made of the election to and qualification for membership of Henry Fitch Blount.

The following circular of the Committee on Publications was then read:

Circular of the Committee on Publications of the Philosophical Society of Washington.

The Committee on Publications of the Philosophical Society of Washington beg to announce to the members of the Society the completion and distribution to members of volume XI of the Bulletin. This volume, containing xxxi–618 pages, has been printed and distributed to members in eighteen parts, at irregular intervals, of which the first was issued in February, 1889, and the last in April, 1892.

Under authority from the General Committee, the Publication Committee has made arrangements for binding this volume at the expense of the Society. The binding is to be in substantial cloth cover and uniform in style. The binding will be done by Messrs. Gibson Bros., 1238 Pennsylvania avenue. Any member having a complete set of the eighteen parts constituting volume XI can have them bound in the style adopted by the committee free of charge.

To enable those members who may have lost or destroyed some of the parts to complete their broken sets single copies of separates will be sold to such members at cost.

The list of these separates and the rate at which one copy may be bought by each member is as follows:

Philosophy and specialties, by Garrick Mallery..........	39 pp.,	$0 15
On the observation of sudden phenomena, by S. P. Langley.	10 "	05
On some of the greater problems of physical geology, by C. E. Dutton..	14 "	05
On the crystallization of igneous rocks, by J. P. Iddings..	49 "	20
On the reduction of pendulum observations, by E. D. Preston..	16 "	10

The relative abundance of the chemical elements, by F. W. Clarke	12, pp.,	$0 05
Assumption and fact in the theories of solar and stellar proper motions, by J. R. Eastman	30 "	15
Hurricanes in the Bay of North America, by Everett Hayden	17 "	10
The mineral composition and geological occurrence of certain igneous rocks in the Yellowstone National Park, by J. P. Iddings	30 "	15
The evolution of serials published by scientific societies, by W J McGee	26 "	15
On certain peculiar structural features in the foot-hill region of the Rocky Mountains, near Denver, Colorado, by G. H. Eldridge	28 "	15
The progress of meteoric astronomy in America, by J. R. Eastman	84 "	40
Money fallacies, by C. E. Dutton	26 "	15
Mohawk lake beds, by H. W. Turner	26 "	15
Constitution and origin of spherulites in acid eruptive rocks, by Whitman Cross	34 "	20
Spherulitic crystallization, by J. P. Iddings	20 "	10
Obituary notices	32 "	15
Proceedings	154 "	50

The price of volume XI, unbound, is $2.50; bound, is $3.00.

Applications for purchase should be made to Mr. W. C. Winlock, Secretary of the Society, Smithsonian Institution.

Members desiring to take advantage of this arrangement for binding should, after seeing that their set is complete, deliver it to Mr. Marcus Baker, United States Geological Survey, as soon as practicable and not later than May 15, 1892, arranged in order, tied together and clearly marked with the name and address of the owner. A blank label for this purpose is enclosed. If the volume is to be sent out of the city the cost of carriage must be paid by the owner of the volume.

The binders will deliver the bound volumes to Mr. Baker, from whom members will receive them.

For the benefit of those members who may prefer a different binding from that adopted by the committee, the binders are ready to make special arrangements. Such arrangements, however, must be made by each member on his own account.

ROBERT FLETCHER, *Chairman*,
W. C. WINLOCK,
MARCUS BAKER,
Committee on Publications.

WASHINGTON, D. C., *April 21, 1892.*

Mr. G. M. SEARLE read a paper on A simple form of double image micrometer.

It was discussed by Dr. GOULD, and Messrs. HARKNESS, PAUL, MENDENHALL, FARQUHAR, WOODWARD, and SEARLE.

Mr. Arthur Keith followed with a paper on The geology of Chilhowee mountain in Tennessee. [Published in this volume, pp. 71–88.]

The paper was discussed by Messrs. Willis and Walcott.

Mr. B. E. Fernow read a paper entitled Timber physics. [Not published. Its substance is contained in various reports and bulletins of the Division of Forestry, U. S. Department of Agriculture.]

This was discussed by Messrs. Harrington and Fernow.

389th Meeting. **May 7, 1892.**

President Gilbert in the chair.

Thirty-one members and guests present.

Mr. J. P. Iddings read a paper on The origin of igneous rocks. [Published in full in this volume, pp. 89–214.]

This was discussed by Dr. Williams, of Baltimore, and Messrs. Cross, Clarke, and Lindgren.

Mr. J. E. Watkins read a paper on John Stevens and his sons, early American engineers. [Published in a pamphlet entitled The Camden and Amboy Railroad; origin and early history, by J. Elfreth Watkins, C. E., etc. 8°, Washington, press of W. F. Roberts (1892, published by the Pennsylvania R. R. Co.), pp. 83–102.]

390th Meeting. **May 21, 1892.**

President Gilbert in the chair.

Twenty-five members and guests present.

Mr. H. A. Hazen read a paper on Scientific ballooning. [Privately printed.]

Remarks were made by Messrs. Woodward, Mendenhall, and Hazen.

Mr. A. S. Christie read a paper on The method employed to find the latitude-variation tide. [Published in Bulletin of the

Philosophical Society of Washington. 8°, Washington, 1895, vol. 13, pp. 103–122. See p. 116 of that paper.]

It was discussed by Messrs. MENDENHALL, HARKNESS, WOODWARD, FARQUHAR, BAUER, and CHRISTIE.

391st Meeting. **October 15, 1892**

President GILBERT in the chair.

Thirty-two members and guests present.

Announcement was made of the death, on July 30, 1892, of Professor EDWARD T FRISTOE.

The exercises of the evening consisted of the reading of obituary notices of deceased members as follows:

ASA OWEN ALDIS, written by JOSEPH K. MCCAMMON and read by the Secretary. [Published in this volume, pp. 433–435.]

Mr. WILLIAM FERREL, written and read by Mr. CLEVELAND ABBE. [Published in this volume, pp. 448–460.]

Mr. J. E. HILGARD, written and read by Mr. O. H. TITTMANN. [Published in this volume, pp. 462–465.]

Mr. C. H. NICHOLS, written and read by Mr. J. M. TONER. [Published in this volume, pp. 477–485.]

Mr. C. C. PARRY, written and read by Mr. F. H. KNOWLTON. [Published in this volume, pp. 497–499.]

Mr. GEORGE BANCROFT, written and read by Mr. J. B. MARVIN. [Published in this volume, pp. 442–447.]

Remarks were made by Messrs. HARKNESS and GOODFELLOW.

392d Meeting. **October 29, 1892.**

President GILBERT in the chair.

Thirty members present.

Papers were then read as follows:

Mr. A. W. HARRIS, on the Agricultural Experiment Station exhibit by the Department of Agriculture at the World's Fair.

Mr. C. D. WALCOTT, Notes on the Lower Paleozoic rocks between the Susquehanna and Potomac rivers. [Published in the

American Journal of Science. 8°, New Haven, 1892, Dec., 3d series, vol. 44, no. 264, pp. 469–482.]

Mr. T. Russell, on The relation of rainfall to river rise. [Published in a book entitled Weather and methods of forecasting. 8°, New York, Macmillan & Co., 1895.]

Mr. William Harkness, on Flexure of telescope tubes. [Not published. Is expected to appear hereafter in the publications of the United States Naval Observatory.]

Remarks were made by Messrs. Woodward, Green, and Harkness.

393d Meeting. **November 12, 1892.**

President Gilbert in the chair.

Twenty-six members and guests present.

Mr. J. P. Iddings read a paper on The geology of Crazy mountains. [Not published.]

It was discussed by Messrs. Turner, Walcott, Cross, Gilbert, Diller, and Iddings.

Mr. H. W. Turner read a paper on The lavas of Mount Ingalls. [Published in the American Journal of Science. 8°, New Haven, 1892, Dec., 3d series, vol. 44, no. 264, pp. 455–459.]

It was discussed by Messrs. Iddings, Diller, Gilbert, and Turner.

Mr. W. H. Dall read a paper entitled Determination of the dates of publication of Conrad's "Fossils of the Tertiary formation." [Published in this volume, pp. 215–240.]

394th Meeting. **November 26, 1892.**

President Gilbert in the chair.

Thirty-five members and guests present.

Mr. B. E. Fernow read a paper entitled Recent contributions toward the discussion of forest influences. [Not published. The

substance of it is contained in various reports and bulletins of the Division of Forestry, U. S. Department of Agriculture.]

This was discussed by Messrs. ABBE, FERNOW, BIGELOW, RILEY, and CHAPMAN.

Mr. ROBERT T. HILL read a paper on The occurrence of iron in Mexico. [Published in the American Journal of Science. 8°, New Haven, 1893, Feb., 3d series, vol. 45, no. 266, pp. 111–119.]

This was discussed by Messrs. TURNER, CROSS, DILLER, HILL, and LINDGREN.

395th Meeting. **December 10, 1892.**

Vice-President FLETCHER presided.

Attendance, about four hundred.

By the courtesy of the authorities of the Columbian University, the meeting was held in the lecture room of that institution.

Mr. G. K. GILBERT, the retiring President of the Society, then delivered the annual address, the subject being The moon's face; a study of the origin of its features. [Published in full in this volume, pp. 241–292.]

396th Meeting. **December 22, 1892.**

TWENTY-SECOND ANNUAL MEETING.

President GILBERT in the chair.

Twenty-four members present.

The minutes of the Twenty-first Annual Meeting were read and adopted.

The annual report of the Secretaries was read and accepted.

REPORT OF THE SECRETARIES FOR 1892.

WASHINGTON, D. C., *December 22, 1892.*

To the Philosophical Society of Washington:

The Secretaries have the honor to submit the following annual report for the year 1892:

The number of active members recorded in last year's report was 197. Since that time 3 members have died—General M. C. MEIGS, January 2; Hon. JOHN JAY KNOX, February 9, and Professor E. T FRISTOE, July 30—7 have resigned, 2 have been dropped, and 5 have been transferred to the absent list; 4 new members have been elected and 4 have been transferred to the active list. The net loss to the active list is therefore 9, leaving the active list at the close of this meeting 188.

The roll of new members for 1892 is as follows:

HENRY FITCH BLOUNT.
FREDERICK HAYNES NEWELL.
WILLIAM BRAMWELL POWELL.
GEORGE ROCKWELL PUTNAM.

The Society has held 15 regular meetings and 1 special meeting for the presentation and discussion of communications. A special meeting was held January 20 for the delivery of the annual address of the retiring President of 1891. Besides these there were the regular meeting for the delivery of the annual address of the retiring President of 1892 and the present annual meeting, making in all 19 meetings held during the year.

The average attendance at the meetings held for the presentation of papers was 33. The least number in attendance at any meeting was 20 and the greatest 75.

During the year 43 communications were presented to the Society by 26 members and 2 guests. Of these communications 2 were presidential addresses and 9 obituary notices.

Seven communications, including 1 presidential address, have been published by the Society and 10 others are in course of publication.

The General Committee has held 17 regular meetings (including the present meeting of December 22) and 1 special meeting during the year 1892, with an average attendance of 13. The least number at any meeting was 8 and the greatest number 17.

The Mathematical Section has held 4 meetings, at which 6

communications were presented and the average attendance was 14.

Very respectfully,

J. S. Diller,
W. C. Winlock,
Secretaries.

The report of the Treasurer was then read, accepted, and referred to an auditing committee, consisting of Messrs. G. E. Curtis, Edward Farquhar, and George P. Merrill.

REPORT OF THE TREASURER FOR 1892.

The Philosophical Society of Washington, D. C.:

The Treasurer submits herewith his annual financial statement, covering the period from December 19, 1891, to December 22, 1892.

The expenses of the year exceeded the receipts by $201.12, the excess being paid from the balance brought forward from last year's account.

The money assets of the Society consist of one $500 United States 4 per cent. bond, No. 64,596; one $1,000 United States 4 per cent. bond, No. 135,639, and thirty-five $100 Cosmos Club 5.20 bonds, Nos. 16, 17, 18, 19, 20, 21, 22, 119, 120, 121, 122, 123, 124, 125, 126, 127, 128, 129, 130, 131, 132, 135, 136, 153, 155, 156, 159, 161, 162, 163, 164, 165, 166, 167, and 185, a total investment of $5,000; all of which bonds are in the locked box of the Society in the vaults of the National Safe Deposit and Trust Company of Washington, D. C.

Dues are owing to the Society as follows: For 1891, $40; for 1892, $225.

There are no outstanding liabilities.

The personal property of the Society consists of one fine mahogany table and chair, one large blackboard, one reading stand, one magic lantern, and one microscope.

Respectfully submitted.

Wm. A. De Caindry,
Treasurer.

December 22, 1892.

DR. *The Treasurer in Account with The Philosophical Society of Washington, D. C.* CR.

1891. Dec. 19	To balance on hand per last account	$1,655 61	1892. Dec. 22	By cash paid during the year on the following accounts:	
	To cash received during the year: For dues of 1889, $5; dues of 1890, $30; dues of 1891, $115; dues of 1892, $695; total	845 00		For printing and binding bulletins	$845 42
				For engraving and photolithographing	71 65
				For stationery, postage, postal cards, circulars, envelopes, and clerical services	224 65
	To cash received as interest on investments, viz.: On $1,500 U. S. 4 per cent. bonds, $60; on $3,500 Cosmos Club 5.20 bonds, $175; total	235 00		For miscellaneous expenses of Secretary, Treasurer, and committees	55 78
				For Directory of Scientific Societies	38 63
	To cash received from sales of bulletins	11 01		For rent of box at National Safe Deposit Co	5 00
				For use of assembly hall, Cosmos Club	51 00
				Total	$1,292 13
				By cash for thirteen Cosmos bonds	1,309 58
			Dec. 22	By cash balance on deposit at Riggs & Co.	144 91
		$2,746 62			$2,746 62

Respectfully submitted.

WM. A. DE CAINDRY, *Treasurer.*

DECEMBER 22, 1892.

On motion of Mr. EASTMAN, it was voted that the present officers of the Society continue to act until the close of this meeting.

Elections were then held in accordance with the rules, with the following result:

President...............G. BROWN GOODE.

Vice-Presidents........ { W. H. DALL. ROBERT FLETCHER. R. S. WOODWARD. F. W. CLARKE. }

Treasurer...............W. A. DE CAINDRY.

Secretaries..............W. C. WINLOCK. J. S. DILLER.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

MARCUS BAKER. H. M. PAUL.
H. H. BATES. RICHARD RATHBUN.
F. H. BIGELOW. C. V. RILEY.
B. R. GREEN. O. H. TITTMANN.
L. F. WARD.

The rough minutes of the meeting were then read, and at 9.42 the Society adjourned.

GENERAL MEETINGS.

1893.

397th Meeting. **January 7, 1893.**

The President, Mr. G. Brown Goode, in the chair.

Fifty-five members and guests present.

The President announced the following standing committees of the Society for 1893:

On Publications:

Robert Fletcher, *Chairman.* W. A. De Caindry.
Marcus Baker. W. C. Winlock.

On Communications:

J. S. Diller, *Chairman.* G. P. Merrill.
J. R. Eastman. E. D. Preston.
B. E. Fernow. Richard Rathbun.
I. C. Russell.

The following resolution was reported by the General Committee:

Resolved, That the General Committee recommend to the Society that it make provision for suitable commemorative exercises on the occasion of the four hundredth meeting of the Society.

It was voted that a committee be appointed to consider the matter and report at the next meeting.

As such committee the Chair appointed J. S. Diller, F. W. Clarke, and W. H. Dall.

Mr. G. K. Gilbert made a communication entitled Illustrations of the physical history of the moon. [Published in this volume, pp. 241–292.]

It was discussed by Messrs. Harrington, Abbott, Dall, Bigelow, Chapman, and Gilbert.

398th Meeting. **January 21, 1893.**

President GOODE in the chair.

Forty-one members present.

Announcement was made of the death, on May 29, 1892, of REUBEN DELAVAN MUSSEY, a member of the Society.

On behalf of the Auditing Committee appointed December 22, 1892, Mr. CURTIS submitted the following report, which was adopted:

REPORT OF AUDITING COMMITTEE FOR 1892.

WASHINGTON CITY, *December 30, 1892.*

To the Philosophical Society of Washington:

The undersigned, a committee elected at the annual meeting of the Philosophical Society of Washington, December 22, 1892, for the purpose of auditing the accounts of the Treasurer, respectfully report as follows:

We have examined the statement of receipts, including dues, interest, and sales, and find the same to be correct. We have examined the statement of disbursements, compared it with the vouchers, and find that they agree.

We have examined the returned checks, which agree with the bank book and with the vouchers.

The balance reported by Riggs & Co. December 22, 1892, viz., $144.91, agrees with the Treasurer's report.

We have examined the United States and Cosmos Club bonds belonging to the Society and find them to be in amount and character as represented in the Treasurer's report, aggregating $5,000.

Respectfully submitted.

GEO. E. CURTIS.
EDWARD FARQUHAR.
GEO. P. MERRILL.

The committee appointed at the last meeting to consider the matter of commemorative exercises in connection with the four hundredth meeting submitted a report, which was adopted and referred to the General Committee to be carried into effect.

Mr. T. C. MENDENHALL made a communication on The use of planes and knife-edges in pendulums. [Published in the American Journal of Science. 8°, New Haven, 1893, Feb., 3d series, vol. 45, no. 266, pp. 144–150.]

This was discussed by Messrs. HARKNESS, PAUL, CHAPMAN, FARQUHAR, and PRESTON.

Mr. R. S. WOODWARD made a communication entitled The use of long steel tapes for measuring base lines. [Published in U. S. Coast and Geodetic Survey Report for 1892. 8°, Washington, 1893, part 2, Appendix 8, pp. 334–503.]

Owing to the lateness of the hour, discussion of this paper was postponed till the next meeting.

399th Meeting. **February 4, 1893.**

President GOODE in the chair.

Thirty-eight members present.

Mr. R. S. WOODWARD read an abstract of his paper presented at the last meeting, and the paper was then discussed by Messrs. PAUL, DALL, FARQUHAR, HARKNESS, ABBE, MENDENHALL, and WOODWARD.

Mr. F. L. O. WADSWORTH made a communication entitled Method of determination of the metre in terms of a wave length of light. [It consisted of an abstract of a special report by A. A. Michelson, entitled Determination expérimentale de la valeur du mètre en longueurs d'ondes lumineuses, which was published in Travaux et memoires du Bureau Internationale des Poids et Mesures, 1894, vol. 11.]

It was discussed by Messrs. MENDENHALL, HARKNESS, PAUL, and WADSWORTH.

400th Meeting (Special). **February 18, 1893.**

The program for this meeting was a special one designed to suitably commemorate the four hundredth meeting of the Society. For its proceedings see pp. 547–562.

401st Meeting. **March 1, 1893.**

Vice-President DALL in the chair.

Seventeen members present.

Mr. WALDEMAR LINDGREN read a paper on Two Neocene rivers of California.

Remarks were made by Messrs. TURNER, DILLER, WALCOTT, and LINDGREN.

Mr. MARSHALL McDONALD made a communication on A study of the Gulf stream in relation to the tile fish. [Never written.]

It was discussed by Messrs. WALCOTT and DALL.

402d Meeting. **March 18, 1893.**

Vice-President FLETCHER in the chair.

Forty-eight members and guests present.

The Chair announced the election to membership of FRANK LAWTON OLCOTT WADSWORTH; LOUIS ALBERT FISCHER, and CYRUS ADLER.

Mr. W. H. HOLMES made a communication entitled Traces of glacial man in the Trenton gravels. [Published in the Journal of Geology. 8°, Chicago, 1893, Jan.–Feb., vol. 1, no. 1, pp. 15–37.]

It was discussed by Messrs. POWELL, HARKNESS, McGEE, and HOLMES.

Mr. ASAPH HALL made a communication on The planet Mars. [Not written for publication.]

Remarks were made by Messrs. GILBERT, EASTMAN, and HALL.

403d Meeting. **April 1, 1893.**

Vice-President DALL in the chair.

Twenty-five members present.

Mr. O. T. MASON read a paper on The philosophy of folk-lore. [Published in the Journal of American Folk-Lore. 8°, Boston, 1891, April–June, vol. 4, no. 13, pp. 97–105.]

Mr. W. H. DALL made a communication on A Miocene climate in arctic Siberia. [Published in Proceedings of the U. S. National Museum. 8°, Washington, 1893, vol. 16, pp. 471–478. Also noticed in the American Journal of Science. 8°, New Haven, 1893, Nov., 3d series, vol. 46, no. 275, pp. 399–400.]

Mr. F. H. BIGELOW exhibited and described A model globe, showing the magnetic forces that produce the diurnal variation of the needle. [Published in Astronomy and Astro-Physics. 8°, Northfield, 1893, Oct., vol. 12, no. 8, pp. 706–717.]

Remarks were made by Messrs. HALL, MENDENHALL, and FARQUHAR.

404th Meeting. **April 15, 1893.**

President GOODE in the chair.

Thirty-three members and guests present.

Mr. B. R. GREEN made a communication on The pyramidion of the Washington monument. [Not published.]

This was discussed by Messrs. ABBE, FARQUHAR, CASEY, HARKNESS, BATES, MENDENHALL, PAUL, GILBERT, DALL, and GREEN.

Mr. CLEVELAND ABBE made a communication on The height of the Aurora Borealis. [Not published.]

Remarks were made by Messrs. DALL, HARKNESS, MENDENHALL, HALLOCK, and ABBE.

405th Meeting. **April 29, 1893.**

Vice-president FLETCHER in the chair.

Twenty-eight members present.

Announcement was made of the election to membership of JOHN HENRY MCCORMICK.

Professor ABBE made a communication on Measurements of the growth of plants with an auxanometer. [Not published.]

Remarks were made by Mr. DILLER.

Mr. Henry Farquhar made a communication on The price of silver. [Not published. Paper on same subject read before the American Association for the Advancement of Science, 1893. For three-line abstract see Am. Ass. Adv. Sci. Proc. of 42d meeting, held at Madison, Wis., August, 1893. 8°, Salem, 1894, p. 355.]

Remarks were made by Messrs. Mendenhall, Atkinson, Harkness, and Farquhar.

Mr. M. H. Doolittle made a communication entitled Is there a criterion of objective reality?

On this paper remarks were made by Messrs. Edward Farquhar, Henry Farquhar, and Professor Abbe.

406th Meeting. **May 13, 1893.**

Vice-president Fletcher in the chair.

Twenty-six members present.

Announcement was made of the election to membership of Timothy William Stanton.

Mr. E. D. Preston read a paper entitled Remarks on the method of reducing the Waikiki observations for changes of latitude, with results. [Published in Publications of the Astronomical Society of the Pacific. 8°, San Francisco, 1893, vol. 5, no. 30; also slightly amplified and published as Bulletin no. 27 of the U. S. Coast and Geodetic Survey. 8°, Washington, 1893, pp. 9–20.]

This was discussed by Messrs. Paul, Harkness, Bigelow, Mendenhall, Henry Farquhar, and Preston.

Mr. F. H. Cushing made a communication on Ancient Pueblo arches.

This was discussed by Messrs. Diller and Mindeleff.

Mr. Cleveland Abbe read a paper on The formation of rain. [Not published.]

The paper was not concluded before the hour of adjournment arrived.

407th Meeting. **May 27, 1893.**

President Goode in the chair.

Thirty-five members present.

Announcement was made of the election to membership of Horace Greeley Van Everen.

Mr. S. P. Langley read a paper on Recent observations in the infra-red spectrum. Illustrated by lantern slides. [Not yet published. For some of the facts presented in this paper see Nature, Nov. 1, 1894, and Report of Brit. Ass. Adv. Sci. for 1894.]

Remarks were made by Messrs. Bigelow, Harkness, Abbe, and Bates.

Mr. G. K. Gilbert read a paper on The average temperature of the earth. [Not published.]

Remarks were made by Mr. Farquhar.

408th Meeting. **October 28, 1893.**

Vice-President Fletcher in the chair.

Seventeen members present.

Announcement was made of the death, on June 14, 1893, of Thomas Antisell, one of the founders of the Society; also of the death, on August 9, 1893, of Thornton Alexander Jenkins, also one of the founders of the Society.

Mr. William Harkness made a communication on The oblique aberration of achromatic telescopes. [Not published. It is expected to appear hereafter in the publications of the United States Naval Observatory.]

On this communication remarks were made by Messrs. Eastman and Baker.

Mr. W J McGee read a paper on Classification—chiefly geological. [This paper was also read before the International Congress of Geologists in connection with the World's Colum-

bian Exposition, at Chicago, in September, 1893. It will be published in the Journal of Geology, Chicago.]

409th Meeting. **November 11, 1893.**

President Goode in the chair.

Thirty-eight members present.

Mr. T. C. Mendenhall made a communication on The electrical congress of 1893 and its work in defining units. [Published in Science. 4°, New York, 1895, January 4, new series, vol. 1, no. 1, pp. 9–15.]

Remarks were made by Messrs. Harkness, Mendenhall, Bigelow, and Wadsworth.

Mr. H. A. Hazen read a paper on The mechanism of a tornado. [Published in the American Meteorological Journal. 8°, Boston, 1893, Nov., vol. 10, no. 7, pp. 301–304.]

Remarks were made by Messrs. E. Farquhar, Paul, Eastman, Mendenhall, Abbe, and Hazen.

410th Meeting. **November 25, 1893.**

Vice-president Fletcher in the chair.

Thirty-two members present.

Mr. F. L. O. Wadsworth made a communication on The manufacture of very accurate straight-edges. [Published in Journal of the Franklin Institute. 8°, Philadelphia, 1894, July, vol. 138, no. 823, pp. 1–20. Republished in The American Machinist, August 2, 1894.]

It was discussed by Mr. Harkness.

Mr. A. B. Johnson read a paper on The cruise of the *Clover*; further investigations into the aberration of audibility of fog signals.

Remarks were made by Messrs. Hazen, Harkness, White, and Green.

411th Meeting. **December 9, 1893.**

Vice-president FLETCHER in the chair.

Twenty-five members present.

Mr. C. A. WHITE made a communication on The cruise of the *Clover*. Results—echoes of fog signals. [Published in Science. 4°, New York, 1894, vol. 23, pp. 56–62; also translated into Spanish and published in full in Revista general de marina. 8°, Madrid, 1894, Julio, tomo 35, pp. 17–25.]

Remarks were made by Messrs. WELLING, WEAD, MENDENHALL, ABBE, H. FARQUHAR, HARRINGTON, JOHNSON, and FLETCHER.

Mr. F. H. BIGELOW read a paper on The influence of solar polar radiation upon temperatures in the United States. [Published in Astronomy and Astro-Physics. 8°, Northfield, 1894, Jan., vol. 13, no. 1, pp. 25–40.]

412th Meeting. **December 23, 1893.**

TWENTY-THIRD ANNUAL MEETING.

President GOODE in the chair.

Thirty-one members present.

The annual report of the Secretaries was presented and accepted. The following is the

ANNUAL REPORT OF THE SECRETARIES FOR 1893.

WASHINGTON, D. C., *December 23, 1893.*

To the Philosophical Society of Washington:

The Secretaries have the honor to submit the following annual report for the year 1893, covering, for convenience, from the 396th meeting, on December 22, 1892, to the 411th meeting, on December 8, 1893, inclusive:

The number of active members of the Society given in the last report was 188. During the year 4 have died, 3 have resigned, 6 have been dropped, and 3 have been transferred to the

absent list; 1 member has been transferred from the absent to the active list and 6 new members have been elected. To the net loss of 9 to the active list during the year should be added 2 not previously accounted for, making a total reduction of 11 and bringing the number of active members on December 8, 1893, down to 177.

The deaths have been:

Thomas Antisell.
Thornton Alexander Jenkins.
William Lee.
Reuben Delavan Mussey.

The new members:

Cyrus Adler.
Louis Albert Fischer.
Timothy William Stanton.
Horace Greeley Van Everen.
Frank Lawton Olcott Wadsworth.

The absent list on December 8, 1893, numbered 66.

Besides the annual meeting and the dinner to commemorate the 400th meeting, there were 14 meetings for the presentation and discussion of scientific papers, at which the average attendance was 33; the attendance at the annual meeting was 24 and at the dinner 89. At these meetings 29 communications were presented by 25 members, and 34 members participated in the discussions.

The General Committee has held 15 regular meetings, with an average attendance of 12 members, the minimum attendance being 7 and the maximum 16.

The Mathematical Section has held no meetings during the year.

The stock of the Society's publications is now stored at the Smithsonian Institution, through the courtesy of the Secretary of the Institution, and is systematically arranged and accounted for. The regular edition of 500 copies each of 9 separates of volume 12 have been received and copies have been supplied to all members entitled to them.

From the sale of publications $12.95 have been received and turned over to the Treasurer.

The Secretaries' correspondence files and files of reports for the past five years have been rearranged for convenient reference.

J. S. Diller,
W. C. Winlock,
Secretaries.

The report of the Treasurer was read and referred to an auditing committee, consisting of Messrs. T. H. BEAN, MARK W. HARRINGTON, and J. H. BRYAN. The following is the

ANNUAL REPORT OF THE TREASURER FOR 1893.

DECEMBER 23, 1893.

The Philosophical Society of Washington, D. C.:

The Treasurer has the honor to submit herewith a statement of the finances of the Society for the period beginning December 22, 1892, and ending December 23, 1893.

The income of the year 1893 was $897.95; the disbursements, $405.60, leaving a balance in favor of the Society on the year's transactions of $492.35.

The following is a list of the Society's investments, the securities for which are on deposit in the Society's box in the vaults of the National Safe Deposit, Loan and Trust Company, viz: one U. S. 4 per cent. bond, No. 64,596, $500; one U. S. 4 per cent. bond, No. 135,639, $1,000; thirty-five Cosmos Club first-mortgage bonds, Nos. 16, 17, 18, 19, 20, 21, 22, 119, 120, 121, 122, 123, 124, 125, 126, 127, 128, 129, 130, 131, 132, 135, 136, 153, 155, 156, 159, 161, 162, 163, 164, 165, 166, 167, and 185, $3,500, amounting in all to $5,000.

The property of the Society consists of one mahogany table and chair, one large blackboard, one reading-stand, one magic lantern, and one microscope.

The assets of the Society are as follows:

The securities on deposit at the Safe Deposit Co., as above	$5,000 00
Cash balance at Riggs & Co., per statement herewith	782 26
Unpaid dues	275 00
Total	$6,057 26

The Treasurer is not aware of any outstanding liabilities except $45 rent of meeting-hall for 1893.

WM. A. DE CAINDRY,
Treasurer.

DR. *The Treasurer in Account with The Philosophical Society of Washington, D. C.* CR.

1892. Dec. 22	To cash balance on deposit at Riggs & Co.		$144 91	1893. Dec. 23	By cash paid during the year 1893 on the following accounts, viz:	
	To cash received during 1893 for dues, as follows:				For printing and binding of publications	$196 49
	Dues for 1891	$10 00			For engraving	25 00
	Dues for 1892	125 00			For stationery, postage, postal cards, circulars, envelopes, and clerical services	137 77
	Dues for 1893	650 00			For Directory of the Scientific Societies	41 34
	Dues for 1894	10 00	795 00		For box rent at Safe Deposit Company	5 00
	To cash received in 1893 as interest on investments, viz:					
	On $1,500 U. S. 4 per cent. bonds	$60 00				
	On $3,500 Cosmos Club 5.20 bonds	175 00	235 00			
	To cash received from sales of publications in 1893		12 95	Dec. 23	By cash balance on deposit at Riggs & Co.	782 26
			$1,187 86			$1,187 86

WM. A. DE CAINDRY, *Treasurer.*

DECEMBER 23, 1893.

The annual election of officers was then held, with the following result:

President	ROBERT FLETCHER.	
Vice-Presidents	MARCUS BAKER. F. H. BIGELOW.	F. W. CLARKE. W. H. DALL.
Treasurer	W. A. DE CAINDRY.	
Secretaries	RICHARD RATHBUN.	W. C. WINLOCK.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

H. H. BATES.	M. W. HARRINGTON.
T. H. BEAN.	H. M. PAUL.
J. H. GORE.	C. V. RILEY.
B. R. GREEN.	O. H. TITTMANN.

LESTER F. WARD.

This was followed by the reading of the rough minutes of the meeting.

GENERAL MEETINGS.

1894.

413th Meeting. **January 6, 1894.**

The President, Mr. ROBERT FLETCHER, in the chair.

Thirty-one members present.

The President announced the following standing committees of the Society for 1894.

On Publications:

MARCUS BAKER, *Chairman*. W. C. WINLOCK. W. A. DE CAINDRY.

On Communications:

F. W. CLARKE, *Chairman*. O. H. TITTMANN. F. H. BIGELOW. E. D. PRESTON.

Announcement was also made of the election to membership of EDWARD BENNETT GARRIOTT, of the United States Weather Bureau.

The committee appointed at the last meeting to audit the accounts of the Treasurer submitted the following report, which was adopted:

REPORT OF THE AUDITING COMMITTEE FOR 1893.

WASHINGTON, D. C., *January 6, 1894.*

To the Philosophical Society of Washington:

The undersigned, a committee appointed at the annual meeting of the Philosophical Society of Washington, December 23, 1893, for the purpose of auditing the accounts of the Treasurer, respectfully report as follows:

We have examined the statement of receipts, including dues, interest, and sales, and find the same to be correct. We have

examined the statement of disbursements, compared it with the vouchers, and find that they agree. We have examined the returned checks, which agree with the bank book and with the vouchers. The balance reported by Riggs & Co. December 21, 1893, $782.26, agrees with the Treasurer's report. We have examined the United States and Cosmos Club bonds belonging to the Society and find them to be in amount and character as represented in the Treasurer's report, aggregating $5,000.

TARLETON H. BEAN.
MARK W. HARRINGTON.

Mr. W. H. DALL made a communication entitled Notes of exploration on the Appalachicola river.

Mr. M. W. HARRINGTON read a paper on The Texas monsoons. [Published in this volume, pp. 293–308.]

This paper was discussed by Messrs. BIGELOW, BAKER, HARRINGTON, and HILL.

Mr. H. A. HAZEN made a communication entitled Ten miles above the earth. [Published in American Meteorological Journal. 8°, Boston, 1894, Feb., vol. 10, no. 10, pp. 423–427.]

It was discussed by Messrs. HARRINGTON, WADSWORTH, and HAZEN.

The President announced that Mr. RICHARD RATHBUN, who was elected Secretary at the annual meeting, would be unable to serve by reason of expected absence from the city, and that Mr. J. S. DILLER had been elected by the General Committee to fill the vacancy.

414th Meeting. **January 20, 1894.**

Vice-president CLARKE in the chair.

Twenty-five members present.

Announcement was made of the election to membership of CHARLES KASSON WEAD.

Mr. F. L. O. Wadsworth read a paper on Fixed spectroscopic trains. [Published in the London, Edinburgh, and Dublin Philosophical Magazine. 8°, London, 1894, Oct., 5th series, vol. 38, no. 233, pp. 337–351; republished in Astronomy and Astro-Physics. 8°, Northfield, 1894, Dec., vol. 13, no. 10, pp. 835–849.]

Mr. I. M. Cline read a paper entitled Summer hot winds on the Great Plains. [Published in full in this volume, pp. 309–348; also reprinted in part in the American Meteorological Journal. 8°, Boston, Ginn & Co., 1894, Sept., vol. 11, no. 5, pp. 175–186.]

This was discussed by Messrs. Farquhar and Harrington.

Mr. A. B. Johnson read a paper entitled The cruise of the *Clover;* further observations on the aberrations of audibility of fog signals.

This was discussed by Messrs. McCormick, Wadsworth, and Johnson.

415th Meeting. **February 3, 1894.**

President Fletcher in the chair.

Forty-five members present.

Announcement was made that C. Hart Merriam had been appointed an additional member of the Committee on Communications; also that the following persons had been named as delegates to the Joint Commission: The President, *ex officio;* Marcus Baker and Bernard R. Green.

Mr. Fremont Morse read a paper on Recent astronomical work in Alaska. [Not published.]

This was discussed by Messrs. Harkness, Farquhar, and Pratt.

Mr. Marcus Baker read a paper on Relief maps. [Published in this volume, pp. 349–368.]

After a recess to allow examination of models, it was discussed by Messrs. Farquhar, Ogden, Court, Doolittle, Kummell, Howell, Green, and Baker.

416th Meeting. **February 17, 1894.**

President FLETCHER in the chair.

Thirty-nine members and guests present.

Announcement was made of the election to membership of JOHN THOMPSON HEDRICK.

Mr. G. K. GILBERT made a communication on A proposed machine for the computation of least square problems. [Not published.]

It was discussed by Messrs. ABBE, HARRINGTON, BAKER, BIGELOW, and GILBERT.

Mr. F. W. CLARKE made a communication on The atomic weight of oxygen; a study in constant error.

This was discussed by Messrs. KUMMELL, ABBE, DOOLITTLE, FARQUHAR, and CLARKE.

417th Meeting. **March 3, 1894.**

President FLETCHER in the chair.

Thirty-nine members present.

The Secretary read a letter from the Geographical Society of the Pacific inviting the Philosophical Society of Washington to be represented by one or more delegates at a Conference of Geographers to be held at the California Midwinter International Exposition, May 4, 1894.

Mr. GEORGE DAVIDSON read a paper on A method of determining the periodic error of a micrometer screw. [Published in U. S. Coast and Geodetic Survey Report for 1892. 8°, Washington, 1893, Appendix 9, pp. 505–513; also separately.]

This was discussed by Messrs. PAUL, BAKER, WADSWORTH, BIGELOW, and DAVIDSON.

Mr. C. F. MARVIN read a paper upon An automatic mercurial barograph, which was exhibited. [Not published. For description of instrument see pamphlet published by the Weather

Bureau, U. S. Department of Agriculture, entitled Barometers and the measurement of atmospheric pressure, 1894.]

Remarks were made by Messrs. HARKNESS and MARVIN.

Mr. G. W. LITTLEHALES made a communication on The efficiency of oil for lessening the violence of breaking waves. [Not published. The substance of the paper was printed on the reverse side of the pilot chart of the North Atlantic ocean issued by the Navy Department for the month of February, 1894.]

418th Meeting. March 17, 1894.

President FLETCHER in the chair.

Thirty-one members and guests present.

Mr. ALEXANDER MACFARLANE, of the University of Texas, by invitation, read a paper entitled The fundamental principles of exact analysis. [Not published.]

This was discussed by Messrs. HARRINGTON, BIGELOW, and MACFARLANE.

Mr. F. L. O. WADSWORTH read a paper on A machine for mechanically analyzing Fourier's "visibility" curve and its application to the solution of least square problems. [Not published. Is expected to appear soon in the Philosophical Magazine.]

419th Meeting. March 31, 1894.

President FLETCHER in the chair.

Twenty-six members and guests present.

Mr. C. H. KUMMELL read a paper on The direct synthetical method of adjusting a triangulation. [Published in U. S. Coast and Geodetic Survey Report for 1892. 8°, Washington, 1894, Appendix No. 12, pp. 535–552; also as a separate, with the date 1893.]

Remarks were made by Messrs. DOOLITTLE, BAKER, and KUMMELL.

Mr. T. Nelson Dale, by invitation, presented a communication entitled A few words on the geology of eastern New York. [Not published. This communication was an abridged account of the matter published in Monographs of the U. S. Geological Survey, volume 23. 4°, Washington, 1894, pp. 119–206; and in the Thirteenth Annual Report of the U. S. Geological Survey. 8°, Washington, 1893, part 2, pp. 291–340.]

This was discussed by Messrs. Van Hise, Gilbert, and Dale.

420th Meeting. **April 14, 1894.**

President Fletcher in the chair.

Forty members and guests present.

Mr. W J McGee read a paper on The antiquity of man in America. [Printed (in part) in the Epitome. 8°, Washington, 1894, vol. 1, pp. 1–8.]

Remarks were made by Messrs. Harkness and McGee.

Mr. Albrecht Wirth, by invitation, made a communication on Some modern African problems.

Remarks were made by Messrs. Adler and Wirth.

421st Meeting. **April 28, 1894.**

President Fletcher in the chair.

Nineteen members present.

Mr. E. D. Preston read a paper entitled Mean density of the earth from two Hawaiian mountains. [Published in this volume, pp. 369–396.]

This was discussed by Messrs. Harkness, Farquhar, and Preston.

Mr. Cleveland Abbe presented a paper on The continental deficit of gravity and its meteorological bearings. [Published in

the American Meteorological Journal. 8°, Boston, 1894, May, vol. 11, No. 1, pp. 1–6.]

This was discussed by Messrs. HARKNESS, GILBERT, PRESTON, and ABBE.

422d Meeting. **May 12, 1894.**

Vice-president DALL in the chair.

Forty-six members and guests present, of whom six were women.

Professor T. C. MENDENHALL made a communication on Gravitation work, and accompanied it by an exhibition of some quarter-second gravity pendulums and accompanying apparatus. [Not published.]

The communication was discussed by Messrs. ABBE, NEWCOMB, BATES, EIMBECK, and DOOLITTLE.

Mr. F. H. BIGELOW made a communication entitled Temperature inversions in the solar magnetic period. [Published in the American Journal of Science. 8°, New Haven, 1894, Dec., 3d series, vol. 48, no. 288, pp. 435–451.]

The paper was discussed by Messrs. ABBE and WEAD.

Mr. ELMER GATES read a paper on The artificial transformation of simple organisms. [Not published.]

The paper was discussed by Mr. WARD.

423d Meeting. **May 26, 1894.**

Vice-president CLARKE in the chair.

Twenty-one members present.

Mr. J. K. MCCAMMON read an obituary notice of ASA OWEN ALDIS. [Published in this volume, pp. 433–435.]

Mr. D. W. PRENTISS read an obituary notice of EDWARD T FRISTOE. [Published in this volume, pp. 460–461.]

Mr. C. HART MERRIAM read a paper on The temperature control of the geographic distribution of plants and animals. [Pub-

lished in the National Geographic Magazine. 8°, Washington, 1894, Dec. 29, vol. 6, pp. 229–238.]

Remarks were made by Mr. G. Brown Goode.

Mr. C. K. Wead made a communication on A possible explanation of the periodic inversion of Bigelow's curve. [Not published.]

This was discussed by Messrs. Bigelow and Mendenhall.

Mr. H. A. Hazen made a communication on Auroras and sun spots. [Published in the American Meteorological Journal. 8°, Boston, 1894, Oct., vol. 11, no. 6, pp. 221–228.]

Remarks were made by Mr. Bigelow.

424th Meeting. **October 27, 1894.**

President Fletcher in the chair.

Twenty-five members and guests present.

Announcement was made of the death of the following members of the Society:

James Clarke Welling, on September 4, 1894.

Robert Stanton Avery, on September 12, 1894.

Garrick Mallery, on October 24, 1894.

Mr. Victor L. Mason read, by invitation, a paper on Modern munitions and engines of war in the United States military service. It was illustrated by photographs loaned by the Bureau of Ordnance of the War Department. [Published in the Century Magazine. 8°, New York, 1895, Feb., vol. 49, no. 4, pp, 570–583.]

Remarks were made by Messrs. Ward, Harkness, Dall, Chatard, Baker, Weed, and the President.

425th Meeting. **November 10, 1894.**

President Fletcher in the chair.

Forty members and guests present.

Mr. S. P. Langley presented a communication on Bolography of the infra-red spectrum; it was illustrated by photographs

and lantern slides. [Not yet published. See references cited on p. 524.]

Remarks were made by Messrs. ABBE, CHAPMAN, H. FARQUHAR, and LANGLEY.

A paper by Mr. L. A. BAUER, on The earliest isoclinics and the earliest observations of terrestrial magnetic force, was read by Mr. ABBE in the absence of the author. [Published in this volume, pp. 397–410. Abstract, with additional matter, published in Nature. 4°, London, 1895, Jan. 24, vol. 51, no. 1317, pp. 295–297.]

Mr. P. A. WELKER read a paper entitled Electrical phenomena on Mount Elbert, Colorado. [Not published.]

Remarks were made by Messrs. ABBE and PAWLING.

A letter, dated October 31, 1894, from John W. Hoyt, chairman of the National University Committee, to the President of the Society, was read by direction of the General Committee. It stated that the committee of the United States Senate charged with the subject of the National University intended to push the pending bill, and suggested that the scientific societies of Washington adopt resolutions and take action in its behalf.

On motion of Mr. WARD, it was voted that the President appoint a committee of five to consider this matter and take such action as it may deem advisable.

As such committee the President appointed Messrs. WARD, CLARKE, BATES, GORE, and ADLER.

426th Meeting. **November 24, 1894.**

President FLETCHER in the chair.

Fifty members and guests present.

The special committee appointed at the last meeting to consider the subject of Mr. Hoyt's letter submitted a report, which was read by Mr. WARD, and, on motion of Mr. FARQUHAR, it was accepted and its recommendations adopted.

REPORT OF COMMITTEE ON NATIONAL UNIVERSITY.

The committee appointed to consider the communication of the Hon. John W. Hoyt relative to the Society's taking action looking toward the establishment of a National University begs leave to report that its members sympathize with the idea of the establishment of the "University of the United States" at Washington, and presume that many of the members of the Philosophical Society share this feeling. The committee is of the opinion, however, that it is neither useful nor expedient for the Society to take any action on this subject as a society. The committee takes the liberty of suggesting that members of the Society interested in the project use their personal endeavors towards bringing about the establishment of the university, and, further, that the Secretary be authorized to reply to Mr. Hoyt in the spirit of the report of your committee, which is herewith respectfully submitted.

LESTER F. WARD.
H. H. BATES.
F. W. CLARKE.
CYRUS ADLER.
J. H. GORE.

Mr. G. BROWN GOODE, President of the Society in 1893, delivered an address entitled What has been done for science in America. [Not yet published.]

427th Meeting. **December 12, 1894.**

By the courtesy of the authorities of Columbian University, the meeting was held in the lecture room of that institution.

Dr. ROBERT FLETCHER, President of the Society, delivered the annual address; subject, Anatomy and art. [Published in this volume, pp. 411–432; also in the form of separates.]

428th Meeting. **December 22, 1894.**

TWENTY-FOURTH ANNUAL MEETING.

President FLETCHER in the chair.

Fifteen members present.

The annual report of the Secretaries was read and accepted.

ANNUAL REPORT OF THE SECRETARIES FOR 1894.

WASHINGTON, D. C., *December 22, 1894.*

To the Philosophical Society of Washington:

The Secretaries have the honor to submit the following annual report, covering from the 412th meeting, December 23, 1893, to the 427th meeting, December 12, 1894, inclusive:

The number of active members of the Society given in the last annual report was 177. During the year 3 have died, 13 have resigned, 4 have been dropped, and 6 have been transferred to the absent list; 4 new members have been elected, and the number of active members December 12, 1894, was 155.

The deaths have been:

ROBERT STANTON AVERY. GARRICK MALLERY.
JAMES CLARKE WELLING.

The new members are:

EDWARD BENNETT GARRIOTT. JOHN THOMPSON HEDRICK.
ELMER GATES. CHARLES KASSON WEAD.

The number of members on the absent list December 12, 1894, was 70.

Besides the annual meeting and the meetings at which Mr. G. BROWN GOODE, President of the Society for 1893, and Dr. ROBERT FLETCHER, President of the Society for 1894, delivered their addresses, there were 13 regular meetings of the Society for the presentation and discussion of papers. At these meetings the average attendance was 35, an increase of 2 over the average attendance of 1893.

Thirty-one communications were made to the Society by 29 members, an increase of 2 communications and 4 participants over last year. The number of those who took part in the discussions was increased from 34 to 38.

The General Committee held 15 meetings, with an average attendance of 11; minimum attendance 8 and maximum 15.

Respectfully submitted.

J. S. DILLER,
W. C. WINLOCK,
Secretaries.

The report of the Treasurer was read and referred to an auditing committee, consisting of Messrs. McGee, Abbe, and Winston.

ANNUAL REPORT OF THE TREASURER FOR 1894.

December 22, 1894.

The Philosophical Society of Washington, D. C.:

The Treasurer has the honor to submit herewith a statement of the finances of the Society for the period beginning December 23, 1893, and ending December 22, 1894.

The income of the year 1894 was $836.05; the disbursements properly chargeable to the year, $642.77, leaving a balance in favor of the Society on the year's transactions of $193.28.

The following is a list of the Society's investments, the securities for which are on deposit in the Society's box in the vaults of the National Safe Deposit, Loan and Trust Company, viz: One U. S. 4 per cent. bond, No. 64,596, $500; one U. S. 4 per cent. bond, No. 135,639, $1,000, and forty Cosmos Club first-mortgage bonds, issue of 1886, as follows: Nos. 16, 17, 18, 19, 20, 21, 22, 45, 70, 119, 120, 121, 122, 123, 124, 125, 126, 127, 128, 129, 130, 131, 132, 135, 136, 153, 155, 156, 159, 161, 162, 163, 164, 165, 166, 167, 185, 193, 194, and 195. These securities amount in all to $5,500.

The property of the Society consists of one mahogany table and chair, one large blackboard, one microscope, one magic lantern, and one iron-frame reading-stand.

The assets of the Society are as follows:

The securities on deposit at the Safe Deposit Co., as above	$5,500 00
Cash balance at Riggs & Co., per statement herewith	595 71
Unpaid dues	270 00
Total	$6,365 71

The Treasurer is not aware of any outstanding liabilities, except a bill of $3.50 for printing.

Respectfully submitted.

Wm. A. De Caindry,
Treasurer.

Dr. *The Treasurer in Account with The Philosophical Society of Washington, D. C.* Cr.

1893. Dec. 23	To cash balance on deposit at Riggs & Co.		$782 26
	To cash received during the year 1894 for dues, as follows:		
	Dues for 1892	$20 00	
	Dues for 1893	140 00	
	Dues for 1894	570 00	
	Dues for 1895	8 50	
			738 50
	To cash received in 1894 as interest on investments, viz:		
	On $1,500 U. S. 4 per cent. bonds	$60 00	
	On $4,000 Cosmos Club 5.20 bonds, 1886	197 50	
			257 50
	To cash received in 1894 from sales of publications		8 55
			$1,786 81

1894. Dec 22.	By cash paid during the year upon the following accounts, viz:	
	For printing	$393 85
	For drawing	20 00
	For engraving	51 70
	For stationery, postage, postal cards, circulars, envelopes, and clerical and other services	59 60
	For Directory of the Scientific Societies	35 22
	For rent of meeting halls	100 00
	For box rent at Safe Deposit Company	5 00
	For repairs to blackboard	22 40
	For purchase of Cosmos Club 5.20 bonds, Nos. 45, 70, 193, 194, and 195	503 33
Dec. 22	By cash balance on deposit at Riggs & Co.	595 71
		$1,786 81

Wm. A. De Caindry, *Treasurer.*

December 22, 1894.

It was, on motion, voted that the present officers continue to act till the close of this meeting.

The election of officers for the ensuing year was then held, with the following result:

President...............	W. H. Dall.	
Vice-Presidents........	Marcus Baker.	F. W. Clarke.
	F. H. Bigelow.	L. F. Ward.
Treasurer...............	W. A. De Caindry.	
Secretaries..............	B. R. Green.	W. C. Winlock.

MEMBERS AT LARGE OF THE GENERAL COMMITTEE.

Cyrus Adler.	M. W. Harrington.
H. H. Bates.	H. M. Paul.
T. H. Bean.	C. V. Riley.
J. Howard Gore.	O. H. Tittmann.

C. D. Walcott.

PROCEEDINGS

OF THE

MATHEMATICAL SECTION.

1892 TO 1894.

FROM THE MINUTES.

65th Meeting. **February 17, 1892.**

The Chairman, Mr. Gore, presided.

Present, nineteen members and guests.

The annual election of officers was held and resulted in the reëlection of J. H. Gore as Chairman and G. E. Curtis as Secretary.

Mr. Asaph Hall made a communication entitled The capture of comets.

Remarks were made by Messrs. Hill and Farquhar.

Mr. R. S. Woodward made a communication entitled The general problem of least squares and a solution of one of the higher special cases.

Remarks were made by Messrs. Hall and Kummell.

66th Meeting. **March 30, 1892.**

Mr. M. H. Doolittle presided.

Six members present.

Mr. C. H. Kummell began the presentation of a communication entitled Symmetries of the cubic and methods of treating

the irreducible case. It being unfinished at 10 o'clock, the hour of adjournment, it was assigned to be continued at the next meeting.

67th Meeting. **April 27, 1892.**

The Chairman, Mr. Gore, presided.

Eight members present.

Mr. Kummell completed the presentation of his paper begun at the last meeting.

Mr. M. H. Doolittle made a communication on Least square adjustments.

Upon this remarks were made by Messrs. Curtis, Woodward, and Kummell.

68th Meeting. **November 30, 1892.**

The Chairman presided.

Twenty-one members and guests present.

Mr. R. S. Woodward made a communication on The rectification of a slightly sinuous curve.

Remarks were made by Mr. Harkness.

Mr. F. L. O. Wadsworth made a communication on The application of interference methods to the measurement of base lines.

Remarks were made by Messrs. Woodward and Paul.

No meetings of the Mathematical Section were held in 1893 and 1894.

PROCEEDINGS OF THE FOUR HUNDREDTH MEETING

OF THE

PHILOSOPHICAL SOCIETY OF WASHINGTON.

This was a special meeting designed to suitably commemorate and celebrate the four hundredth meeting of the Society.

It was held on the evening of February 18, 1893, at 7.30 p. m., at the Cochran, corner of Fourteenth and K streets northwest, and consisted of a banquet, with toasts and reminiscences.

The committee of management was Messrs. DILLER, CLARKE, and DALL.

President GOODE presided.

Eighty-nine members and guests were present, of which the following is a complete list (names of founders in bold-faced type):

Cleveland Abbe.
Cyrus Adler.
W. H. Ashmead.
Frank Baker.
Marcus Baker.
T. H. Bean.
H. G. Beyer.
F. H. Bigelow.
J. S. Billings.
Rogers Birnie.
J. M. Browne.
J. H. Bryan.
E. S. Burgess.
S. M. Burnett.
T. M. Chatard.
J. W. Chickering.
F. W. Clarke.
Whitman Cross.
G. E. Curtis.
W. H. Dall.
W. A. De Caindry.
J. S. Diller.
R. E. Earll.
J. R. Eastman.
G. H. Eldridge.
S. F. Emmons.
Edward Farquhar.
O. L. Fassig.
B. E. Fernow.
Robert Fletcher.
E. M. Gallaudet.
Henry Gannett.
G. K. Gilbert.
Theodore Gill.
G. B. Goode.
J. C. Gordon.
J. H. Gore.
B. R. Green.
A. W. Greely.
William Hallock.
William Harkness.
R. A. Harris.
Everett Hayden.
C. W. Hayes.
H. L. Hodgkins.
W. H. Holmes.
E. E. Howell.
G. G. Hubbard.
H. M. Hurd.
Arthur Keith.
A. F. A. King.
F. H. Knowlton.
G. W. Littlehales.
Alexander McAdie.
Marshall McDonald.
J. H. McCormick.
W J McGee.

Garrick Mallery.
B: P. Mann.
J. B. Marvin.
O. T. Mason.
T. C. Mendenhall.
Cosmos Mindeleff.
Simon Newcomb.
W. L. Nicholson.
H. G. Ogden.
H. M. Paul.
A. C. Peale.
W. M. Poindexter.
J. W. Powell.
W. B. Powell.
D. W. Prentiss.
E. D. Preston.
C. V. Riley.
C. A. Schott.
T. W. Smillie.
A. H. Thompson.
O. H. Tittmann.
J. M. Toner.
A. C. True.
F. W. True.
F. L. O. Wadsworth.
C. D. Walcott.
J. E. Watkins.
J. C. Welling.
W. C. Winlock.
H. C. Yarrow.
W. S. Yeates.

Mr. Mitchell, reporter for the Washington *Post*, was also present.

The guests of the evening were:

William Harkness, President of the American Association for the Advancement of Science.

O. T. Mason, President of the Anthropological Society.

C. V. Riley, President of the Biological Society.

Gardiner G. Hubbard, President of the National Geographic Society.

W. H. Ashmead, Vice-President of the Entomological Society.

Garrick Mallery, President of the Joint Commission.

Letters of regret were presented from:

Chief Justice M. W. Fuller, Chancellor of the Smithsonian Institution.

Professor S. P. Langley, Secretary of the Smithsonian Institution.

Professor O. C. Marsh, President of the National Academy of Sciences.

Hon. W. T. Harris, Commissioner of Education.

F. P. Dewey, President of the Chemical Society.

Of the founders of the Society *eight* were present. Their names are printed in bold-faced type in the preceding list. Of the remainder Thomas Antisell, S. V. Benét, and T. A. Jenkins were absent by reason of illness. Asaph Hall, J. G. Parke, and W. B. Taylor, although in Washington, were unable to attend. B. F. Greene responded from West Lebanon, N. H. From T. L. Casey and from G. H. Elliott there was no response. Of the forty-four founders of the Society but *seventeen* * are living.

Dinner ended, the President said:

We commemorate today the founding of the oldest of the scientific societies of Washington—the foster-mother of all which have since come into existence—the successor and the heir of those which had preceded.

* Perhaps this number should be *eighteen*, the committee having no knowledge of J. B. Wheeler.

The organizations which are represented around this board are not entirely of recent growth. They are the fruit of efforts which have continued for nearly a century.

Ninety years ago, when Washington was a mere village in the midst of the virgin forest, it had a society where scientific and economic questions were discussed; where Jefferson and Barlow and Fulton and Law and Culbrody and Meigs and Adams met to plan for the scientific progress of the nation.

Eighty years ago the old Columbian Institute was born, and this for nearly thirty years maintained the scientific interests of the city.

Its successor was the National Institute, organized in 1840, which was for a time the most powerful scientific body on the continent, and which for two decades continued to hold meetings and to publish transactions.

There followed a period of disorganization and one of recovery, and then, shortly after the civil war, the Philosophical Society was born.*

Only twenty-two years have passed, and Washington has become the most important scientific center in the Americas; twenty-two years, and instead of one society we have six; *only* twenty-two years, and but few are left among a membership of nearly two hundred who were connected with the Society in those early days.

We have with us tonight eight of the founders of our Society. We shall listen with great interest to what they have to tell us of the past.

Gentlemen, I greet you at this *our four hundredth meeting.*

To the toast *Our first president*, J. C. WELLING responded, passing in review the work accomplished by Professor Henry for the Philosophical Society while serving as its President, 1871–1878.

The toast *Our four hundredth meeting* was responded to by J. S. BILLINGS, who said:

I appreciate highly the honor to be called on to respond to this toast, which I take to mean the Society as it was, as it is, and as it will be. Many of you, no doubt, remember the meetings in the old Ford's Theatre, on Tenth street. The entrance up the narrow stairs, often pervaded with a scientific odor from the laboratory on the lower floor—an odor once compared to that of the deluge at low tide—the devious and complicated route from the head of the stairs, past the General Committee room, to the place of meeting; the rather gloomy room, walled in from floor to ceiling with books from whose dingy backs no light was reflected, and yet in its general aspects and surroundings in many respects appropriate to the objects and purposes of the company gathered therein. A few of you can remember the days when Professor Henry presided, and his custom of taking a five minutes' dip into one of the volumes of the large series

* From 1861 to 1871 the only meetings of scientific men in the city were those of the Saturday Club and the Potomac Side Naturalists' Club.

of Transactions of the Royal Society, which occupied the shelves behind and on the right of his chair, by way of refreshment before calling the Society to order.

Perhaps half a dozen here present were also present at the first meeting for organization, in March, 1871, in the Regents' room at the Smithsonian Institution, and may remember the discussion as to whether occasional refreshments in the shape of crackers and beer would or would not promote the objects of the Society.

I hope that most of you are familiar with the main points in the first anniversary address on the character and object of the Society, made by Professor Henry in 1871, even though you may not have been present at its delivery. He said that its objects were to be the consideration of positive facts and laws of the physical and moral universe, rather than speculation as to possibilities, and that it was to be a local establishment, arrogating nothing to itself on account of its position at the capital, and making no claim to being in any respect a special representative of the science of the country. He said: "It is of the first importance that the operations of this Society be conducted with great care, and that nothing be given to the world under its sanction which is not based upon thorough investigation of established scientific principles. We should be warned by the fate of a society established in this city some thirty years ago, which, although it included among its members a few men of true science, was under the control mainly of amateurs and politicians, and therefore was unfit to discharge the duties which it claimed as one of its functions, to decide questions of a strictly scientific character."

In the main, the Society has continued to work on the lines indicated in this address. Counting by years, it is yet young, only twenty-two years old, yet what changes in its field of observation and in its surroundings can be noted by its few surviving founders as they look back over this brief period. Each of you in his own line of study and work knows of the advances and changes. I will refer to but one, the one with which I am most familiar, the science of bacteriology, which has been wholly developed within the last twenty years, and which is opening up a new branch of chemistry and leading to new methods in preventive medicine and therapeutics.

But if the Society is young in years, she is full grown and has a number of flourishing children—biological, anthropological, chemical, geological, and so on—all of whom we consider as part of the family, and from whom we expect to hear in the course of the evening. Some of her children and members have been a little impatient at times with the old lady, thinking her a little slow and unprogressive, and that her general style of housekeeping was not as brilliant as it might be, but she has gone on in a steady respectable fashion, and has always been in the best society.

Science is becoming fashionable of late years, and, as a natural consequence, a sort of scientific *demi-monde* has been evolved, but it has not yet been made at home in the hall of the Philosophical Society. We old gentlemen of the last generation prefer that this should be so. We may,

in the spirit of scientific investigation, secure good seats to inspect the latest patterns of skirt-dancing and high-kicking in the places where these are something of a specialty, but we don't want them at home.

The four hundredth meeting. The Society as it is. I hope that each member present is serenely confident that this is the best of all possible societies, flourishing in the best of all possible worlds. If any one doubt it, let him turn to his neighbor and have his doubts removed. I have no time to spare to argue this point, and will only congratulate you on its present standing among learned societies, its membership, its bank account, and its future prospects. This brings me naturally to the third head of my discourse, the Society as it is to be. This is mainly a matter for the younger members, and I congratulate them on the possibilities that are before them. It should be pleasant to them to reflect that the older ones have not done what they intended to do when they were young—that is, to discover and explain everything, to provide all possible formulæ, to classify and name all minerals, fossils, and living things. Just think what a melancholy earth this would be for you if they had done all this, whereas now you can have no end of enjoyment in correcting the errors and supplementing the deficiencies of their work.

Besant says that "old people have most of the wealth of the world, and they make believe that with the wealth the world itself belongs to them; but it is a fond delusion. When the power to fight, to create, to make, is gone, the power to enjoy vanishes as well. The old people can creep about in their gardens and their houses, can put on robes of authority, can give orders to servants, but the round world and all that therein is and is worth having belongs, has belonged, and always will belong, to the young."

This dictum needs a qualification, viz., that the terms "old" and "young" are not to be understood as referring merely to years of life; but with that modification it may pass.

Nevertheless, we, the ancients, feel as deep an interest in the progress which is being made and in the prosperity of this Society as we ever did when we were busiest in its affairs.

There are some occasional troubles connected with the process of growing old, but it is the only way that has yet been discovered of living a long time, and there are certain tastes which, once formed, do not decay. Many studious men begin to lose somewhat of their receptivity after fifty, to be happy in the prospect that there are many things that they need not know, and that they may take pleasure in many things which are not subjects of logical demonstration.

"Happy is the man who has gained a knowledge of the causes of things and trampled all fear under foot and risen above relentless fate and the hungry clamor of death. Yet not less blest is he who knows the rustic gods, even Pan, and old Silvanus and the sister nymphs."—*Virgil.*

But he who has once tasted the pleasures of original observation and research never loses his memory of them. When circumstances compel him to put them aside he can appreciate the explorer's restlessness after a period of quiet—the "prairie thirst"—the "India thirst" of which

Tommy Atkins tells, "If you've 'eard the East a callin', you won't never heed naught else."

And so on this anniversary we can all join in these aspirations and desires, we can appreciate the lilt and swing of the song of the old trail by the man who wants

> "To see the old stars wheel back once more
>   And blaze in the velvet blue,
> Where the blindest bluffs hold good, my friends,
>   And the wildest tales are true.
> And the men bulk big on the old trail, our
> Own trail, the out trail, and life runs large
>   On the long trail, the trail that is always new."

To the general toast, *Our sister societies*, GARDINER G. HUBBARD responded on behalf of the National Geographic Society.

On behalf of the Anthropological Society O. T. MASON responded. He said:

I am delighted to have the honor of sitting down with you this evening in this beautiful wigwam around your annual feast.

Your President has asked me to respond to the toast *The Anthropological Society of Washington.* As I have been thinking over the relationship between our two societies and the position which the Philosophical Society holds among the scientific associations of Washington, I am reminded that there has existed in all tribes of men the custom of keeping up a perpetual fire in or around the chief's house or in some special place apart. We are told also that the extinction of this fire portended the direst calamities. Among the American wild tribes, in the pueblo region, in the city of Mexico, or in the capital of the Incas; upon the steppes of Siberia, among the negro tribes of Africa, wherever the sense of reverence was found, there men erected a holy of holies, upon which the divine presence was believed to dwell in a perpetual flame.

The Bantu tribes of South Africa have a custom, when they set up a new kraal or village, of taking fire from the old chief's hearth to kindle their own.

Both among the Greek and early Latin races, at the founding of a new colony, fire was solemnly sent from the prytaneum, or Vesta temple of the mother colony, to kindle a similar sacred fire in the new settlement.

In the middle ages, in Catholic countries, on Holy Saturday (before Easter) all lamps in each church were extinguished and the Paschal candle was lighted with the help of flint and steel. From this sacred source the other lights in the church were kindled, and the various households in the parish took a flame to relight their own fires and lamps.*

The ancient Israelites in their wanderings were accompanied by a pillar

* Encyc. Brit., xxiv, 193.

of smoke by day and a pillar of flame by night, led and accompanied everywhere by their sacred fire.

The same is true of ancient Greece, but in later times the prytaneum contained the sacred fire and the personified goddess of the *ἑστία*, or hearth, became Hestia.

Every Greek state had its prytaneum, which may be described as the town hall of the capital. When the king of Athens extended his sway over the whole of Attica, each petty town, hitherto independent, had to abolish its sacred fire and the hearth of Athens was the prytaneum of Attica. In this building ambassadors were entertained and distinguished citizens maintained at public expense, and it was the headquarters of the officials known as prytanes.

In Rome the goddess still kept the name of Vesta and her sacred edifice was the Ædes Vestæ; but the fire was called *focus*. Vestal virgins were charged with its preservation and punished by being buried alive if it were allowed to go out. They were held in the highest honor and relieved from all public duties and taxations.

From time immemorial it has been the custom with those who have dedicated their lives to the service of truth also to erect in every town a council-house, in the center of which was kept burning the sacred fire of science, and every man who was privileged to enter the holy precincts there received the gift of a live coal, with which he could kindle the flame in his own bosom. At stated times these reverend men would divest themselves of the dress and fashions of daily occupation and betake themselves to their consecrated retreat. They discussed the mysteries of numbers, the brightness and movements of the heavenly bodies, the secrets of fire, and sunlight and lightning and alchemy. They named all the herbs and assigned to each its peculiar properties, and all the animals, reading the inmost intents of their thoughts. But most of all did they ponder upon their wonderful selves—on birth and growth and death; on the body and the soul; on the origins of their tribes; on the mysteries of the under world. There gathered medicine men and astrologers and magicians and sorcerers and Chaldeans and rain-makers and wise men and princes and judges and captains and governors and counselors and sheriffs and all the rulers of provinces to make their reports and to hear and be heard.

It was in the year of our Lord 1871 that such a temple was dedicated in Washington and such an altar, sacred to pure science, was erected. It is not a part of my function now to eulogize those master spirits that gathered round that council fire. Following the customs of the ancient tribes, the walls of the wigwam were decorated with the trophies of past victories; this time not with real skulls and shields and scalps, but with brains. Tier after tier of volumes in gory bindings rose above their heads, in which were pictured the mangled bodies of men and women and children and animals; and here and there, on a pedestal or on the wall, were the busts and portraits of men who had distinguished themselves as carvers of human flesh.

In 1875 I was so happy as to be adjudged worthy to sit down in the outer row of the elect, among the lay brethren; to deposit my hat and overcoat in the ante-chamber; to adjust my collar, and to keep silent while my elders spake. This awful place was in the Army Medical Museum, on Tenth street. It had previously been the temple of religion, was afterward dedicated to the Muses, but now it was the national medicine lodge. The great sachem, Joseph Henry, presided with imperial dignity. The dim lights burning in that ancient estufa shone upon his countenance, and all that sat in the council, looking steadfastly on him, saw his face as it had been the face of an angel.

I remember as though it were yesterday where I used to sit, next to the wizard in cabalistic geometry, General Alvord, and around was war Captain Dutton, the ingenious sachem Bates, the astrologer Harkness, and the reverend prophet Taylor.

The somniferous atmosphere of that sacred wigwam at this very moment seems to invade my nostrils, in spite of the delightful odors of this feast. Tier after tier of ancient writings rise before my imagination, and the authors leap forth from the red and green bindings as they used to do years ago upon my waking dreams. Some of these authors were armed with knives and saws and bandages, while others were equipped with mortars and pestles and galipots, and still another set flourished their sheets of war records. At times the authors of the books and the readers of papers got mixed in my imagination, and the gentleman who had the floor seemed to be sawing and pounding away at their communications like faithful students of medicine. There were several members who invariably went to sleep, and one of them, whose name I never learned, had the comical habit of snoring his applause or disappointment of the papers that were read.

It was in the year 1879 that three of the members of this sacred order of the Cosmos went forth to set up a new kiwa or council chamber in the south tower of the Smithsonian Institution. There was the great medicine man, Dr. Toner; the learned picture-writer and gesture-speaker, Colonel Mallery, and myself. We were soon followed by the distinguished war chief, Major Powell, who had lost his right arm in defending the tribal hunting grounds; the famous arrow-maker, Dr. Robert Fletcher; the learned herb doctor, Professor Ward, and others whom I do not recall.

From the delightful associations of the Tenth-street wigwam these men went forth to set up a new prytaneum, to establish a new hestia for science. There was a rumor of schism and whispers of treason and prophecies of disaster, but not one of these men lost his affection for the old "*focus*" and the parent Society. They only did what has gone on since the birth of society. They borrowed a portion of the central fire, they kindled a new fire elsewhere, they returned at stated times to receive fresh impetus from the Ædes Vestæ, and sought to become in themselves the center of new colonies devoted to the worship of truth.

Since 1879 other bands of young warriors and sachems have repeated the process. The little one has become a thousand, and the small one a strong nation.

In responding to the toast assigned me I desire to renew the vows of fealty of the Anthropological Society to the Philosophical Society. May the fire upon the parent altar never go out, or even grow dim. May her children come again and again to rekindle their affections, and as all studies end in philosophy, it is my prayer that every one of the societies that have come forth from the Philosophical may become in turn the source from which its most energetic membership may be recruited. As in the olden time, whenever a new tribal or subtribal or family establishment was set up there was kindled in the center of the chief's hut a new and sacred fire to Hestia or her prototypes with coals from the parent hearth, so in these latter days, with the same loyal spirit a band of your sons went forth in 1879, my dear Mr. President, and erected a new sacrificial altar to science in the south tower of the Smithsonian Institution. The sparks that kindled our fire were taken from the old hearthstone around which we have to this day loved also to worship.

In primitive Greece the sacred fire was also that of the household, whereat was cooked the family meal day by day. How opportune, therefore, this banqueting together, whereat each filial society, through its representatives, returns to kindle anew the zeal within his own breast.

The extinction of the fire in the temple of Vesta at Rome was regarded as the greatest misfortune that could befall the state. Indeed, we are told that it portended the destruction of the empire. No less calamity would befall this modern capital on the quenching of the flame in the lamp of science. For this reason this Society has sacredly guarded and fed this flame for twenty-two years. For this reason she has sent forth the younger societies with her benediction and watched their progress with the tenderest solicitude.

It is therefore as an expression of undying attachment to the dear old parent Society founded by Professor Henry, whose altar flame was kindled from his own exalted mind, that I beg you all to drink with me this toast, praying that the Philosophical Society of Washington shall dwell in the land wherein the fathers have dwelt, even they and their children and their children's children forever.

For the Biological Society C. V. Riley responded. He said:

The Biological Society, like the others here represented, recognizes itself as an offshoot of the Philosophical Society. It had its inception in a meeting in November, 1880, of a number of persons interested in biology who met in response to a call signed by the honored President of your own Society who now presides at this meeting, and myself. Excepting C. E. Dutton, Ernest Ingersoll, and W. H. Patten, I believe that every one of the gentlemen present at that preliminary meeting is here tonight. An organization was effected November 26 of that same year, and the meetings have been regularly held from that day to this. During our

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On the subject of the prytaneum consult J. G. Frazer, Jour. of Philol., Lond., xiv, 145.

earlier years we met in the lecture-room of the National Museum, and, as is so often the case in the history of societies, the meetings during the first two or three years after organization were perhaps more numerously and more enthusiastically attended than they have been since. The history of the Society fully justifies the wisdom of organizing as an independent body. At present we have 195 members, of whom 154 are active and 44 corresponding. From the beginning it was decided not to make much effort to publish an elaborate volume of proceedings, on the ground that, as most of the members were connected in some way with the scientific work of the Government, the more important communications would find other avenues for publication. It thus results that the proceedings have been confined very largely to the publication of the annual addresses of the presidents, which are delivered at a special meeting following the annual meeting. It has become an unwritten law of the Society that the president shall be reëlected for a second term, and hence it comes that during the twelve years of its existence the Society has had but six presidents, and after honoring in this way the more illustrious of its members has been obliged to turn to the less deserving. Our first president was Theodore N. Gill, who served us during 1881–'82. Following him at intervals of two years were C. A. White, G. Brown Goode (your present worthy president), W. H. Dall, Lester F. Ward, and C. Hart Merriam.

If I should attempt to indicate the characteristics which have more particularly distinguished the Biological Society, I think I might say, without boastfulness, that no other organization has done more during the time of its existence for American biology. Its work has been remarkable also for its original character. Thus in reference to our extreme northwest Pacific coast and our Alaskan possessions we have been particularly fortunate in the amount of information that has been received, and this will not surprise you when I say that, in addition to special papers by others, including Dr. Merriam and Mr. Palmer, we have had repeated communications by Messrs. Dall and Henry W. Elliott, giving the results of their rich experiences in these regions. In ichthyology the work of the Society has also been most interesting, not only from the economic side, but from the systematic, for we have had a large number of communications from members of the Fish Commission, and particularly a number of learned and critical papers on the classification of fishes by Dr. Gill. In paleontology the communications have been equally rich and original, as might be expected with men like White, Dall, Walcott, and others to draw from on the zoölogical and Professor Ward from the botanical side, while Dr. White dealt more particularly in his annual addresses with certain phases in the geological history of the North American continent, biologically considered, and with the application of biology to geological history. In conchology we have had not only some notable communications, but Mr. Dall's annual addresses dealt with the conditions under which deep-sea mollusks exist and with the work and writings of the more notable American conchologists. In botany we have had many notable papers, from the economical and horticultural

side as well as from the purely classificatory. In fact, the botanists have been so strong among us that they have formed a section of the Society which has held independent meetings. Of late years the number of original announcements and discoveries in mammalogy and ornithology has been quite a feature of our meetings. In this and in the question of the geographic distribution of life in North America Dr. Merriam has been most prominent. I may say also, incidentally, that entomology has not been neglected. Another feature of the meetings has been the amount of labor given to the history of the beginnings of natural history in America, most elaborately and interestingly set forth by Professor Goode in his two annual addresses. The subject of taxonomy has been frequently dealt with, and by none more ably than by Dr. Gill, who made this and the principles of zoögeography the subjects of his annual addresses.

It goes almost without saying that the work of the Biological Society, covering a period since Darwin's death, so pregnant with discussion on the origin and development of life upon our planet, has been strongly evolutional. The derivative origin of existing forms has not only been persistently pressed, argued, and exemplified by original observations, but has had a most worthy exposition in the several remarkable papers and the presidential addresses by Professor Ward. Our first volume of proceedings contains also the papers read at a special Darwin memorial meeting.

In short, and not to weary you, the Society has found in the exchange of the experiences, discoveries, and opinions of the many different specialists who are doing original work connected with the Government the fullest justification for its existence, and has proved a worthy scion of a worthy parent. I recollect very well in the earlier meetings of the Philosophical Society, before Professor Henry had left us, that, as a sort of reaction from the rather heavy proceedings, some of us were in the habit of repairing to a certain restaurant in the neighborhood of the old Army Medical Museum and continuing the session in a less formal way over a glass of beer or a plate of oysters. This social and gastronomic phase of the meetings was both beneficial and enjoyable, and there is a growing feeling on the part of members of the Biological Society that perhaps there is at the present time a need of some such feature, an after-meeting social stimulus, to bring members more closely together and make them better acquainted; for I think it is true of us, as it is of other societies, that the younger members are in this way more rapidly made to feel at home and to join in the actual work of the Society. The want is more felt now that we meet in the assembly hall of the Cosmos Club than it was when we met in the lecture-room of the National Museum, because some of the members of the Society who are not members of the Club feel a certain constraint which would be removed in the manner suggested.

Mallock's telling *mot* in "The New Republic" has often been quoted, to the effect that whether life is worth living depends upon the liver; and I may say in conclusion, Mr. President, that while our mother Society

whom we honor tonight may, in her all-embracing scope, soar into the abstruser realms of mathematics and metaphysics, some of us biologists have a feeling that there is not much worth considering in this world that does not concern life upon it.

Response on behalf of the Chemical Society was made by F. W. Clarke, and on behalf of the Entomological Society by W. H. Ashmead.

Responding for the Joint Commission, Garrick Mallery read the following

PHILOSOPHIC PHANTASY.

All ante-bellum Washington was gay
With duels, cock-fights, politics, and play,
And 'twas the aim of all the residents
To live on poker, punch, and Presidents.
Their clubs were fitted for Kentucky colonels,
Their reading limited to party journals.
Little cared they for any kind of knowledge
That didn't count in the Electoral College.
Then every Congressman thought he could boss over
A fellow who was merely a philosopher,
And if a man could write, was white, and thus a free gent,
He was at once marked out to be Smithsonian Regent.

Indeed, o'er all the earth in days gone by
The tree of knowledge grew by no means high.
Our easy grandsires in those happy eras
Bothered no whit on *fin de siècle* chimeras.
They knew all things when everything was new,
Science was not and facts were very few.
Those simple souls, yet smacking of the ape,
From evolution's toils had sure escape
When any bard or scald a lay would tell
Of Chronos, Odin, Ammon, Pthah, or Bel,
And Huxley's prose had proved of scanty use
Fighting an ode to Artemis or Zeus!
Thunder called not for meteoric lore,
When Thor and Vulcan kept its bolts in store.
What need of tomes on earthquake's awesome shocks
If Titans' kicks explained the tumbling rocks?
Nature's vast problems answered each itself
Through monad, pixy, undine, gnome, or elf,
And solved complete was every portent odd
By new editions of the storied god.
E'en at the worst the bard could turn his strain
To Delphic strophe or Sibylline refrain

In verse where any sense—or none—was stowed,
Precursing Browning's or Walt Whitman's mode.

But now the cosmologic drama 's o'er,
Mithra 's a myth, Great Pan pans out no more.
Our world gives little scope to doctrine mystic—
'Tis wary, doubting, stern, and realistic;
Takes every axiom on strict probation,
And calls for *propter hoc* and demonstration.
So e'en in Washington when war was o'er
Some students dared essay the modern lore—
And reckless of its strange ubiety
Founded the Philosophical Society.
No more will Hercules' and Juno's fray
Serve them as reason for the Milky Way.
With calculus and spherics now they grope
And ply the transit and the spectroscope;
Or if they in another branch enlist,
They bother with conglomerate and schist,
Erosions, monoclinals, hypogenes,
Jurassics, mesozoics, miocenes,
    The argillites and trilobites,
    The kyanites and oolites,
    And staurolites and diorites—
But now I find my task is too vexatious
To catalogue granitic and cretaceous.
I'd need the metrical address of Horace
To cap a perfect rhyme with ichthyosaurus,
And though in dactyls I might blythely sing,
A pterodactyl's quite another thing.

They took all fields. Their mathematic skill
Could square the circle or a plumber's bill.
Chemists could analyze a Congress speech,
State what was "ring" and what was Buncombe screech.
Another pundit, if you listen, shows
All that you wish or anybody knows
    About the tribes of Lo's,
    Cheyennes and Crows,
    Crees and Arapahoes,
Red Cloud and Spotted Tail and Roman Nose,
And notes on Crazy Horse, comparing lore
To prove he was the Pegasus of yore;
That Sitting Bull to Minotaur is kin,
Though hid without a clue he saved his skin.
Our sacred Sessions truest rapture draw
When mutual admiration is the law,

And members hint with equanimic pride,
What stupid fools are all the world outside,—
How Spencer's laurels would dry up and pale,
And Darwin droop an atavistic tail!
Your duty's plain, to tell in turn each friend
How well *his* learning and his judgment blend,—
With tacit right while paying court'sy's debt,
To think *you* write a little better yet.
But cursed be he whose jealous tongue instills
Doubt of the wisdom of his brother Phils!
Swear every paper 's full of lore and profit,
Though lead in weight and drier e'en than Tophet!

But while for rivals we prepare the killings,
Let 's joy in cooings here,—likewise in Billings,
And 'mid our joys yet humbly make avowal
Our pow-wow 'll never do without our Powell!
Newcomb and Eastman hint of rising stars,
And Hall once kindly gave twin moons to Mars.
Our Abbe(y) rears a vane that wind foretells,
And 'round us rings a Peale of merry Bells.
We own gay Ho(l)mes, however plain our dwelling,
And must keep well while always keeping Welling.

Old father Saturn, as the myth is told,
Ate up his children ere they grew too old;
But this Society with bounteous care
Brought forth a brood that try to eat their *mère*
The Anthropops of course were cannibals,
Biologers claimed all the animals,
Geographers grabbed air and land and tide,—
And sharps in drugs and bugs stole all beside.

Scared by this coming internecine wreck,
All hands essayed all arts the doom to check;
And we tried divination,
Haruspiciation,
Alectryomances
And hypnotic trances,
And fakir round dances,
And the garter that fell in the Ross' séances,
The watchword of strikes by the Carnegie artisans,
And the harmony twixt Grover C. and the partisans.
We conned then the newspaper pharmacopœia,
And dosed with Pond's Extract and Swaim's Panacea;
Hall's Sicilian Renewer and Barrett's Restorer,
Radway's Ready Relief and McClain's Dermadorer.
We boiled down in cauldrons things heavy and sound,
Patent Office reports, Coast Survey volumes, bound;

Presbyterian pandects, H. George's mistakes;
Free-silver statistics and boarding-house cakes;
And conjured with photos of chorus-girl witches,
And Vinnie Ream's Lincoln and Washington's breeches.

Our land twelve years ago was saved from scission,
And so was Hayes, by forming a Commission.
So, too, a Joint Commission of societies
Saved science and restored the lost proprieties.
Though representatives, we 're not those Reps
Who swear and spar around the Speaker's steps,
Nor yet a Senate, or at least that body
Made up of dollars, terrapin, and toddy.
What though our formal dinner may be horrid,
The soup be frigid and the salad torrid,
Though then your palate gets not full fruition,
You have some pabulum in your Commission;
We 're fit to eat,—if doubting, only try it—
Our body 's in a double sense your Diet!

Rathbun is safe for farinaceous food—
We'll surely get some very Brown and Goode!
Gannett—though not a goose—our taste will whet
As rich a dish as Hayden Ever-et!
Fresh vegetals will surely store our cupboard,
While we can boast a Gardiner in Hubbard.
Since water's Riley now, we turn with glee
Where simmers Mason labelled as O Tea.
But never fear our throats becoming gluey,
We can't be dry while always getting Dewey.

Wiley our ways, yet in a careful guard
We'll hold the fort with dynamics and Ward
And lots of ordnance deadlier far than guns—
Our Clarke's a patent mitrailleuse of puns;
Or if we need to try a sharper hit,
Fletcher provides exhaustless darts of wit.
"Fear boys with bugs," says Shakspeare, but our Howard
With bugs can fright a man who 's not a coward.
From our high teachings we must never fall,
Not only mendin' some, but Menden(h)all!
For this grand quest all we High Joints embark us,
So let our Secretary, Baker, Mark-us!

For *The Presidents of the Philosophical Society*, J. W. Powell responded and prophesied that new societies would be formed in Washington, and that ultimately all the scientific societies would be united and form a confederacy.

Responding to the toast *Auld Lang Syne*, W. H. DALL said that—

In common with most of the older members of the Society, he had felt that its greatest value and usefulness to its members, especially the younger men, arose not so much from the papers bearing on his own special lines of study, important and suggestive as they may have been, as from the opportunities which the meetings have afforded of becoming acquainted with the results of research in other lines. Though not a mathematician or physicist, yet few, even of the most abstruse, papers but had afforded some new and welcome additions to his stock of knowledge, some broadening of mental boundaries, some enlargement of the intellectual horizon. So, too, he had profited by that contact with the riper minds and wider experience which the older and more eminent founders of the Society had brought to its deliberations, and which had been so justly alluded to by Professor Harkness. In recalling the memory of those whom all scientific men delighted to honor, such men as Henry, Dr. Woodward, and many others whose names would occur to every member, he felt a sense of personal obligation for the kindly and wise interest which those eminent men always showed in the work of the younger members. He recalled his last interview with Professor Henry—had when the speaker was on the point of sailing for Europe, and the good old man was already confined to the room which he never left again. The benediction which he received from that Nestor in science in parting had always been remembered as in the nature of a consecration to honesty, persistence, and thoroughness in scientific work.

The speaker recalled also the deep interest felt by Professor Henry in the work of the Society and the method by which he succeeded in bringing to its meetings many workers, not members, who described their progress in lines of research of the highest importance, at times when the results of these investigations were still unknown to the scientific world. One especially notable occasion should not at this time be forgotten. Called by Professor Henry to assist in advance of the meeting in arranging the apparatus of a gentleman then unknown outside the circle of his friends, but now familiar to every cultivated person in both hemispheres, he witnessed the arrangement of wires from one room to another in the old Medical Museum, and after the paper had been delivered the members were invited to leave their seats and listen at an orifice provided in a box placed on the President's table. As one by one made the experiment some thought that they could distinguish spoken words, though, personally, the speaker confessed that all he distinguished was a faint tinkling as of wires in vibration. Yet this was the first public exhibition of the speaking telephone, since so marvelously perfected and distributed over the civilized world; this was the humble seed from which so mighty a tree had sprung. If the Society had never done anything else to deserve distinction, the record of this incident alone would never let its memory die.

Adjourned at 12 o'clock.

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